

Antiquity

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Antiquity

VOL. XXXIV No. 134

JUNE 1960

Editorial

THE death of Sir Leonard Woolley on 20 February, 1960, at the age of 79 took away from us yet another of the small band of distinguished British archaeologists who did so much, mainly between the two Great Wars, to establish in this country a high tradition of technique and discipline. He was born in 1880, the year which saw Schliemann working at Orchomenos, Flinders Petrie beginning work in Egypt and Pitt-Rivers starting his excavations in Cranborne Chase; and to our younger generation, Woolley seemed to have around him something of the aura of the heroes of an earlier archaeology, and to call back the romance and excitement of Layard, Rawlinson, Schliemann and Petrie and the days when archaeology itself was young. He will be remembered as a great excavator, but also as one who could write simply and clearly for a wide public. His *Ur of the Chaldees*, and *The Sumerians*, and, most of all, his *Digging up the Past*, first published in 1930 (and later reissued by Pelican Books in 1937), were noteworthy essays in the very difficult art of archaeological *haute-vulgarisation*.

For us, however, one of Woolley's most interesting and delightful popular books was *Spadework*, a sort of archaeological autobiography first published in 1953, and we reread it when we heard he was dead. It contains two of the finest archaeological anecdotes ever. The first is the story of how Leonard Woolley became an archaeologist at all, and it is summarized in two sentences: 'I have seldom been more surprised than I was when the Warden of New College told me that he had decided that I should be an archaeologist. . . . I must confess that when the prospect did present itself, not as a mere idea to be played with (for one did not lightly play with the Warden's decisions) but as something definite and settled, I was not altogether happy about it.' The second is the story of how Woolley dug for the first time—at Corbridge. Haverfield had agreed to supervise the excavations there and Woolley was put in charge of them. 'In point of fact I had never so much as seen an excavation,' he writes. 'I had never studied archaeological methods even from books (there were none at that time dealing with the subject), and I had not any idea of how to make a survey or a ground plan: apart from being used to handling antiquities in a museum, and that for only a few months, I had no qualifications at all.' Woolley records his disappointment at the fact that Haverfield only looked in at the excavations one day a week, but his pleasure at the fact that the plans were being done by W. H. Knowles. He concludes, 'The work went very merrily . . . and we were all . . . happily unconscious of the low standard of our performance, nor did anyone from outside suggest that it might have been better: actually of course it did improve with time, as both we and our workmen learned our job.'

Thus began a great archaeological excavator; the decision of a Warden and the luck of

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Corbridge—luck that produced the Corbridge Lion. Woolley's reluctance to become an archaeologist was that for him 'archaeology meant a life spent inside a museum, whereas I preferred the open air and was more interested in my fellow men than in dead-and-gone things.' As it happened, his life was to be spent in the open air and he always thought of himself as essentially a field archaeologist. He did not understand by this phrase what Crawford meant, using as Crawford did the English archaeological usage of Williams Freeman. Indeed, when Woolley came to review Crawford's *Archaeology in the Field*, he wrote 'I must admit that in one respect this book gave me a rather unpleasant shock. For the best part of fifty years, since I gave up museum work for digging, I have called myself a field archaeologist, and now Dr Crawford assures me that "field archaeology does not cover excavation"' (*Observer*, 22 February, 1953).

Disputes as to what constitutes field archaeology apart, Woolley, like Crawford, had a clear and inspiring vision of the perspectives of archaeology. He wrote 'the real end of archaeology is, through the dead and gone things, to get at the history and minds of dead and gone men.' These were the sort of words which could have been found in *Man and his Past* or any editorial in *ANTIQUITY* from 1927 to 1957.



Although prehistoric archaeology has been in existence as a scientific humanity for nearly a hundred and fifty years, very few prehistorians have, as yet, felt that they had both the archaeological scholarship and the philosophical and historical background to essay an account of the new perspective of man's past which prehistory provides—or appears to provide to us—at the present day. Two such essays have, however, appeared in English in the last few months. The first is 'Perspectives in Prehistory', his Presidential address to the Prehistoric Society by Professor J. G. D. Clark (*Proc. Preh. Soc.*, 1959, 1), and, the second, Pierre Teilhard de Chardin's *The Phenomenon of Man* (London, Collins, 1959, 25s.). In his Presidential Address, Grahame Clark discusses the varying approaches to the study of antiquity, warmly commending, as we all do, the way in which Gordon Childe broke down the insularity of British prehistory, but commending even more warmly the contribution of O. G. S. Crawford. He quoted the words of Crawford on the first page of the first number of *ANTIQUITY*. 'Our field is the Earth, our range in time a million years or so, and our subject the human race . . .', and adds that this 'was no idle gesture.' Clark went on to say, 'This is a view of prehistory I think we would all do well to entertain because, however limited as to time and place our own interests in prehistory may be, our studies will gain immeasurably if we only see them in the widest perspectives. Personally I would go further and admit that for me prehistory is only worth pursuing because it sets not merely history but contemporary life in the kind of perspective allowed, and in the final resort necessitated, by modern science.'

But what really are these perspectives of prehistory—these widest perspectives? One wonders what Childe and Crawford and Woolley would have thought of *The Phenomenon of Man*, or for that matter what contemporary archaeologists think of this work of Teilhard de Chardin, for the book has been reviewed hitherto mainly by non-archaeologists. *The Phenomenon of Man* was written by Teilhard de Chardin in Peking between June 1938 and June 1940 and revised with a Preface dated Paris, March 1947, and an Appendix dated Rome, October 1948. This last year was the one in which he was forbidden by his Church to put forward his candidature for the Professorship in the Collège de France vacant by the resignation by age of his friend and mentor the Abbé Henri Breuil. It was widely known in Paris, in France as a whole and in England that he would probably have been elected to

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this post and his non-candidature must have been a heavy blow to him. Even heavier was the refusal by Rome in 1950 of permission to publish the book now presented to us in English. In 1951 Teilhard de Chardin moved to New York to work with the Wenner-Gren Foundation. He died in 1955 at the age of 74; his friends and admirers set up committees which are publishing his writings posthumously. *Le Phénomène Humain* was published in Paris in 1955 by *Les Editions du Seuil*, and now appears in English with a preface by Sir Julian Huxley.

Teilhard de Chardin's publishers in France and England describe *The Phenomenon of Man* as 'one of the outstanding intellectual events of the century' and 'a key book of our time'. In his preface to the English edition Sir Julian Huxley describes it as 'a very remarkable work by a very remarkable human being'. In *Le Figaro Littéraire*, André Rousseaux declared that 'not since the 12th century has there been such a satisfying exposition of the perfect understanding between the material world and that of Christ Incarnate', and Arnold Toynbee was sure the book gave 'our generation something that is sorely needs'. On the other hand, Alasdair MacIntyre in the *Guardian* described the book as 'monumentally dull' and said, with reason, that 'the attempt to turn the concept of evolution into a metaphysical key to the universe is one of the graveyards of the intellect, and the present work is merely one more testimony to that fact.'

Everyone interested in human history ought, I am afraid, to read *The Phenomenon of Man*: all who knew of this Jesuit palaeontologist and archaeologist will want to know his views on the perspective of prehistory. Let no one suppose it is easy reading. To begin with, Teilhard de Chardin has invented a new language with words like *hominization* and *complexification*, *planetization* and the *noosphere*, and the translator has had to wrestle with these neologues as well as the complexities of metaphysics. And the book is, by its very nature, out of date; the main text was written over ten years ago, before radio-carbon dating had produced the new prehistory we are all learning at the moment. And there are strange statements which were inaccurate even ten years ago like the declaration that America was first peopled in Neolithic times.

Let us allow all these difficulties. What is the core of Teilhard de Chardin's observations? His overall thesis seems to be that he is describing the evolution of the world from the primal stuff of the universe through life to consciousness and man, and its probable future progression to the omega point. To him 'Religion and science are the two conjugated faces or phases of one and the same act of complete knowledge,' and this was the point of view set out by Breuil in his Huxley Memorial Lecture for 1941 when he said 'Faith and Science in well-balanced minds have ceased to conflict, and correspond to two orders of reality which our minds attain by different, but not adverse, routes.'

These are fine words, but Teilhard de Chardin's book does not show us how he understands, or wants us to understand, the interaction or conjugation of religion and science into the same act of knowledge. Here is the first great disappointment of a thoughtful book written by a priest with a very wide scientific knowledge and experience. The second and major disappointment is that, in the end, we seem no wiser about prehistoric perspectives than when we started. Teilhard de Chardin quotes Breuil as saying to him: 'We have only just cast off the last moorings which held us to the Neolithic Age.' If this means what it appears to mean we ought now to be able to set out history in the perspective of the Palaeolithic, Mesolithic and Neolithic, but no one seems able to do it satisfactorily. Certainly not Teilhard de Chardin in *The Phenomenon of Man*, which, for all its publicity, is vague, woolly, unconvincing, and dull. Teilhard de Chardin made a brave effort at what he might have called perspectivization (to offer him another of his horrid neologues), but his failure should not dissuade others from attempting to set the phenomenon of human physical

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evolution and prehistorical cultural development in their widest biological and historical contexts.

The prehistory of man is not yet integrated into the general study of human history; we have a vista down the long corridors of time, but no real perspectives which the intelligent layman can appreciate. If comparable attempts to that of Teilhard de Chardin are not made from time to time archaeology will become a deadly dull story of potforms and striking platforms. To quote Woolley again, 'The importance of our archaeological material is that it throws light on the history of men very like ourselves . . . and this enlightenment is not merely for the specialist. . . . The opening up of the world affects us all, becomes part of the general intellectual inheritance. . . . The justification of archaeology is that it does in the end concern everyone.'

Of course it concerns everyone, and must be made to concern everyone. As we write these words, speeding across France in the *Barcelona Express*, our disappointment at not lingering in the Dordogne or walking on the *causses* tempered by the delights of seeing the megaliths of Gerona in a day or two, it seems to us that the problems of synthesis and communication are still very rarely being met by professional archaeologists. A splendidly successful example was Stuart Piggott's *Scotland Before History*, and so, in many ways, was Richard Atkinson's *Stonehenge* and his *Stonehenge and Avebury*. But the problems of getting scholars to write clearly and lucidly and simply yet authoritatively for a wide public of professional and general readers are great, as all editors will know. One of the immediately successful ways is the graphic representation of the past by picture and map and this is why we commend in the highest possible terms the recently published *Atlas of the Classical World* (Nelson, London and Edinburgh, 1959, 70s.). This indispensable book, first published in Amsterdam as the *Atlas van de Antieke Wereld* is a successor to the already well known *Atlas of the Bible* (edited by Grollenberg) and the *Atlas of the Early Christian World* (edited by Ven der Meer and Christine Mohrmann), and reviewed in an earlier number of this journal (ANTIQUITY, 1959, 222). It is a beautiful production and with its clever interweaving of text, photographs and maps produces a guide to the phenomenon of classical man which is immediately understandable by all. Here is *haute-vulgarisation* at its very best—organized scholarship displayed in an authoritative and readable way.

Is it too much to hope that Messrs Nelson and their continental associates can organize for us an *Atlas of the Prehistoric World*? Such a work is badly needed, and might do more for the real understanding of prehistory than many a lucubration on perspective and phenomenon, including these present train-jerked words. We must get life back into the aridity of taxonomy and typology, man back into the forefront of a subject which is becoming a dry study of tool-types and type-tools. It was no accident that Woolley called one of his books *Dead Towns and Living Men*, no accident that he insisted that the study of dead and gone things was only valuable if it led to an understanding of dead and gone men. What was it Ophelia sang?

*He is dead and gone, lady,
He is dead and gone;
At his head a grass-green turf,
At his heels a stone.*

Let us not forget, though all we study in antiquity is dead and gone, there is more than the grass-green turf at the head of the men who are dead, and more than the stones at their heels. Otherwise we are no better than the collectors of the 18th century, even though we can lay up to our souls the flattering unction of being scientific.

The Multiple Brush

by JOHN BOARDMAN

John Boardman, formerly Assistant Director of the British School of Archaeology at Athens and then Assistant Keeper in the Department of Antiquities at the Ashmolean Museum, is now Reader in Classical Archaeology in the University of Oxford.

IN all periods artists have admitted to varying degrees the use of 'mechanical aids'. I want here to discuss one which was particularly favoured by the Greeks in the 8th and 7th centuries B.C., although it is to be met also in many other parts of the ancient world as we shall see. It is the multiple brush.

This is a device which any vase-painter concerned with covering large surfaces with regular designs is likely to find attractive. Schoolboys writing 'lines' with two or more pens tied together would recognize the technique. The patterns produced by a comb or similar toothed implement on a vase or the plaster of a wall could readily prompt the invention, and Frankfort¹ has pointed out that a wooden burnisher might become worn away so that its heavy grain left such a comb-pattern. What is surprising is that the brush was not used more often, for it appears on very few ancient wares, and then generally for rather crudely applied decoration most comparable with the coarsely incised comb-patterns. Except, that is, in Geometric Greece, where it was wielded with some skill and ingenuity, often even to good effect, and became so popular in some wares or workshops that it can be said to dictate the use of certain common patterns. The part it played in ordinary Geometric vase-decoration has generally been overlooked or not recognized, and the different ways it is used in different places within and outside Greece make it worthy of more attention than such a minor point of technique might seem to deserve. In FIG. 1 I have sketched some of the commoner, simple patterns for which it was employed on Greek vases.

Its earlier use can be briefly described; as I have said, it was generally for crude all-over patterns. Such form the main element in the decoration of many Predynastic Egyptian vases of the 'Decorated' class, and the multiple wavy line, recalling the hieroglyph for water, naturally became a feature on vases with ships painted on them² (PLATE IX (a)). Braidwood³ says that the same technique is a feature of other Near Eastern wares in the same period, from Syria (Amouq Phases G/H) to Iran. This is not easily corroborated from publications which rely too much on drawings of pottery. A good photograph is needed for the detection of the multiple brush, and it is easy to be deceived by parallel lines drawn carefully and individually. Although many early vases from Mesopotamia and Iran carry patterns which are obviously inspired by comb-decoration, I can find none on which a multiple brush was certainly employed.

In Bronze Age Cyprus there are many examples. On the Middle Bronze Age red-on-black vases and the Late Bronze Age white-on-black vases⁴ (Base Ring II) it is used for

¹ *Studies in Early Pottery of the Near East*, II, 74.

² *Ibid.*, I, pl. 13.1, where the design is drawn out. The 'sail' has been suspected.

³ In R. W. Ehrich *Relative Chronologies*, 40, and *Mounds in the Plain of Antioch* (O.I.P., XLVIII), 7.

⁴ E. Sjoquist, in *Problems of the Late Cypriot Bronze Age*, 42, has the lines in groups only 'roughly parallel' but this is not so.

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haphazard all-over patterns (PLATE IX (*b, c*)). This great similarity of technique, and the light on dark scheme, argues more connexion between these two wares than is generally admitted. On White Painted IV it appears in much the same manner. Its first appearance for more complicated geometric patterns is on White Slip I and II vases where it is used for the cross-strokes of the characteristic step-ladder patterns, and elsewhere (PLATE IX (*d*)).

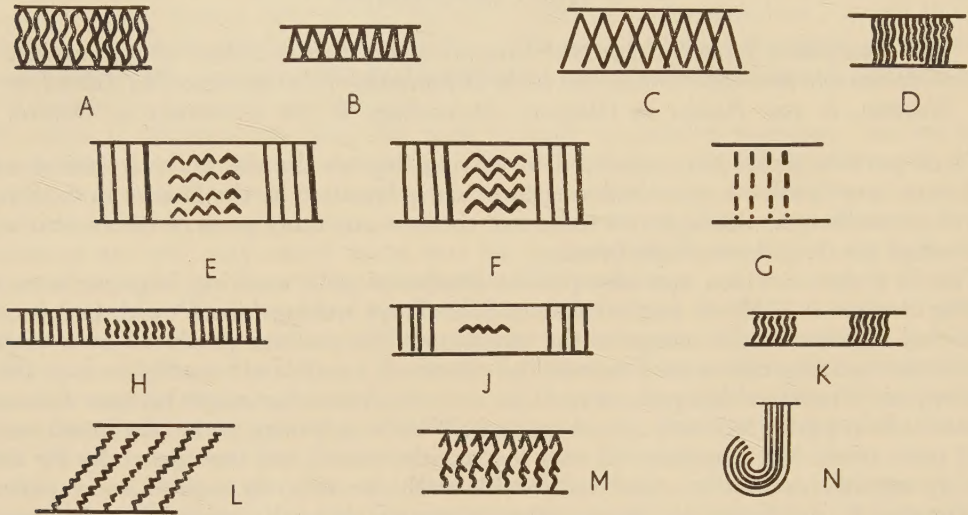


Fig. 1. All these patterns are such as could be made by a quadruple brush, but for H (octuple), K (sex-tuple), and L (brush with 10 members). For N a quintuple brush would be used on a compass, partly free-hand.

In the Aegean the story is quite different. The multiple brush was never used on Helladic or Mycenaean⁵ vases, although the patterns on many of them would seem to invite it. I know of one doubtful example on a Late Minoan IA vase from Gournia.⁶ The well-known Minoan tortoise-shell ripple pattern of Middle Minoan II to Late Minoan I⁷ looks as though it demands a multiple brush but this is illusory. The technique of this fairly common decoration has yet to be satisfactorily explained, but it seems that the lines were painted individually, though with the greatest care to keep them regular and with a consistent rhythm. Before the paint was quite dry a sponge was drawn across the pattern to give the characteristic smudgy effect, and incidentally to remove paint from some parts of each line in so regular a manner as to suggest the use of a multiple brush.

Apart from the quality of the potting and the new system of decoration there are two important technical innovations which mark off the Protogeometric style in Greek vases from all that went before. These are the use of the multiple brush and the compass. In effect they are one, not two, for the multiple brush is *only* used on the compass and never

⁵ The vase quoted by Kübler in *Kerameikos*, I, 141 as a Mycenaean example of the use of a compass and with multiple brush is from a Geometric grave; see *Archaiologike Ephemeris*, 1891, 27, pl. 3.1.

⁶ Good photographs in C. Zervos' *L'Art en Crète*, fig. 426.

⁷ Cf. *Palace of Minos*, I, 593. Furumark's explanation of the technique in *Mycenaean Pottery*, 423 f. cannot be correct.

THE MULTIPLE BRUSH

freehand, although there are many Protogeometric patterns for which it could well have been employed besides the usual concentric circles and semicircles.⁸

Through the Early Geometric Period down to nearly the end of the 9th century B.C.⁹ the multiple brush is still not used freehand, and thus far the story in Greece is much as it is in the Near East. In Cyprus, and in Syria and Palestine where the technique may be earlier, the brush is used only on the compass for concentric circles, not freehand, and not even for the broader hoops so common on Cypriot flasks.¹⁰ In the 8th century, when the Greek vase-painter had realized the possibilities of its use for more varied patterns, Cyprus remains unmoved. What is most surprising is that it was never used on amphora-necks and craters where there are panels framed at either side by groups of vertical lines—a 'metope pattern' much favoured later in Greece and ideally suited to our brush. This Cypriot stubbornness is of some value for it makes possible the attribution to Greek, rather than Cypriot, hands of a small class of cups made at Al Mina (Posideion) in North Syria towards the end of the 8th century.¹¹ A special use of the brush, freehand *on* the compass, may be mentioned here. It produces pendent hooks whose lower parts (on the compass) are concentric three-quarter circles (as FIG. 1, N). They are found in Cypriot vases and were imitated in Rhodes.¹² Thence the motif spread north and into Anatolia, where it is found at Smyrna and in Lydia. While here we may notice the apparent absence of its use in Phrygia, where even the small concentric circles seem often (? always) to have been drawn separately.¹³

The painters of the great Attic Geometric 'Dipylon' vases of the mid-8th century were generally too fastidious to use the multiple brush and executed their meticulous patterns without its aid. Occasionally we find it helping out in a row of zigzags or narrow band of upright wavy lines where it is betrayed by the faulty joins between each group (as FIG. 1, A, B). In general it can be said that good Attic painters used it seldom and carefully. Its only use for figure-drawing comes at the end of the century in a pattern borrowed from Corinth, the files of match-stick water-birds which, with a multiple brush, can be mass-produced in batches of three, four or five (as FIG. 1, M).¹⁴ At the end of the century it is far more widely used for subsidiary ornament. On the hydria in PLATE XI a brush with 12 members was used for the seven subsidiary friezes on neck, body and foot.¹⁵ On the smaller mass-produced cups of the Late Geometric Period it is particularly common, especially for what is generally known as the metope pattern in which groups of upright lines frame various simple motifs, all executed with the multiple brush (as FIG. 1, E, F). A good example is shown in PLATE X (a). Here a quintuple brush is used for the vertical lines in the handle zone, for the patterns between them, and for the motifs on the inside and outside of the lip; these last look like ordinary zigzag lines but are in fact five chevrons painted simultaneously and run together. The reserved band at the lip of cup or crater will often betray

⁸ Desborough, in *Protogeometric Pottery*, 62, 136, 190, remarks on the use of the same multiple brush on different vases. It is not used for hatched patterns as on the vases, *ibid.*, pls. 6 top left, 8 centre.

⁹ Some of the earliest examples of free-hand use seem to be on vases such as those shown in *Kerameikos*, v, 1, pls. 30-1, 44.

¹⁰ I discuss Cypriot practice in this period in *Anat. Studies*, ix (1959), 166 f.

¹¹ *Ibid.*, 163 ff.

¹² K. F. Johansen, *Exochi*, 155-61, figs. 223-4. The origin of the motif may lie in the free-hand hooks on 'Cappadocian'-Hittite vases, as Frankfort, *op. cit.*, II, pl. 10.5

¹³ E. Akurgal, *Phrygische Kunst*, pls. 1-31, *passim*.

¹⁴ Cf. *B.S.A.*, XLII, 147, fig. 6b, birds 5 at a time, a 14-membered brush for the side lines; the Analatos hydria, *B.S.A.*, xxxv, pl. 38b, birds in 3s; *ibid.*, pl. 42a, birds in 4s. K. F. Johansen, *Les Vases Sicyoniens*, pl. 17.1, for Protocorinthian birds in 4s.

¹⁵ This is one of three hydriae by the same artist who used the same multiple brush on each; *B.S.A.*, xxxv, pls. 44-6.

the type of multiple brush used, for the painter often dabbed around it, as with a fork on pie-crust, leaving groups of dashes.

The metope pattern enjoyed a great popularity on Late Geometric vases, and not only in Attica. In the Cyclades and islands of east Greece cups and skyphoi decorated in this way are commonplace; the patterns were allowed to cover as much of the surface of the vase as suited Geometric taste, yet with the minimum of effort on the part of the painter. It is not too much to say that the technique dictated the decoration of these humbler vases, just as it did the subsidiary decoration of many larger vases.

The record of the multiple brush outside Attica may be considered in a little more detail. When the Athenian painter was first beginning to exploit its use the Corinthian was evolving the new co-called Protocorinthian style which is characterized by the repetition of finely drawn and simple Geometric patterns. The brush plays such a large part in these patterns that we are bound to reconsider the praise often accorded the Protocorinthian artist for his finesse. Thus, on a fine kotyle like PLATE X (b), the decoration between the handles required only two steady and one unsteady strokes of a multiple brush with 17 members. Brushes with up to 28 members can be detected on vases of this sort. On a kotyle from the Potter's Quarter in Corinth¹⁶ the painter has dabbed his multiple brush all over the vase in a most un-Greek manner more reminiscent of the prehistoric vases from other parts of the ancient world which we have already discussed. The version of the metope pattern on kotylai reappears in narrow friezes on many larger vases, and was learnt from Corinth by other workshops. It is interesting to note that the close-set horizontal lines on PLATE X (b) and similar vases are painted individually, presumably with the vase turning slowly on the wheel. For these lines a multiple brush seems never to have been used. The same effect was reproduced regularly in Cyprus, rarely in Greece, by painting a shallow spiral, also while the vase was on the wheel; but this is even a lazier solution.

In the Argolid the large, crudely painted craters of the later 8th century offer abundant examples of the brush, although sometimes it is eschewed, not by the best, but by the least careful of the vase-painters.

On another group of non-Attic vases of the latest 8th and 7th centuries it appears, sometimes with only two or three members, for broad friezes of upright wavy lines. These are the grave-amphorae of Boeotia, Euboea and certain of the islands (Paros, Thera) and the friezes are generally to be found on their cylindrical or splaying necks.¹⁷ In these centres too, particularly Euboea and the islands, it is much used for the small cups with metope patterns which we have already discussed. From vases like these the practice spread like a rash over the Etruscan and Italo-Greek vases of the seventh century.¹⁸ PLATE X (c) may have been made in Boeotia or Euboea¹⁹ and makes great play with a sextuple brush, and a double and treble brush on the foot. On the Parian cups and vases patterns with lines of dots seem to have been favoured, and on a group of Subgeometric vases which have on slight grounds been attributed to Siphnos the brush is much used for subsidiary ornament and for the zigzags which appear in the field of figure scenes. The latter usage is only found in Attica about 700, but is seen on a slightly earlier, strongly atticising vase found at Eretria (Euboea).²⁰

¹⁶ K.P. 182; unpublished. I am indebted to Prof. J. L. Benson for a photograph and permission to mention the piece.

¹⁷ E.g. R. Hampe, *Frühe griechische Sagenbilder*, pls. 18-9; *B.S.A.*, XLVII, 16 f. pls. 3-4; *Thera*, II, 199-204 figs.

¹⁸ Cf. A. Akerström, *Der geometrische Still in Italien*, *passim*.

¹⁹ *B.S.A.*, LII, 17, pl. 2c.

²⁰ *B.S.A.*, XLVII, pl. 3A for the Eretria vase. For the use in latest Attic Geometric see *B.S.A.*, XLII, pls. 20-1. Here, as in other notes, I have given easily accessible examples only; they could be multiplied many times over.

THE MULTIPLE BRUSH

The later history of the brush in Greek vase-painting was slight, for the emphasis now was on figured decoration or floral patterns. There is an interesting survival of the metope pattern in Chios. There, the normal Late and Sub-Geometric skyphoi were decorated with it, as in the other islands. During the 7th century a new shape was evolved in which the lip grew higher, the foot narrower and taller. The final form is the Chian chalice with high spreading walls and conical foot. The handle zone of the Geometric skyphos occupied half the height of the whole vase; on the chalice it was about one-fifth only, but the metope pattern still appeared in it until the mid-6th century, as on PLATE X (*d*). Later still, in Athens again, we find that the jejune rays at the base of kotylai of Corinthian type may be no more than a network of thin lines painted with a multiple brush.

Modern standards of publication being what they are I hesitate to assert that yet another factor need be considered in descriptions of painted pottery. But at least it should not be entirely ignored and archaeologists who offer only drawings would do well to observe what nuances they may be concealing. The limitations and possibilities presented by a particular technique are as apparent in vase-painting as they are, for instance, in bronze-work, and the multiple brush was not in the stock in trade of all vase-painters, nor used by all in the same way. Deductions of archaeological value have been drawn from observations of this type of detail, and it may be that the story of the multiple brush will be found to have wider significance than I have claimed for it in this brief survey of its use in Greece and elsewhere.

Book Chronicle

We include here books which have been received for review, or books of importance not received for review, of which we have recently been informed. We welcome information about books, particularly in languages other than English and American, of interest to readers of ANTIQUITY. The listing of a book in this chronicle does not preclude its review in ANTIQUITY.

THE FACE OF THE ANCIENT ORIENT. By SABATINO MOSCATI. pp. 328, 32 plates, four text figures and a map. London, Routledge, Kegan Paul and Vallentine, Mitchell, 1960. 30s. A general introduction to the history and archaeology of the most ancient East.

ANTHROPOLOGY IN THE SOUTH SEAS. Edited by J. D. FREEMAN and W. R. GEDDES. New Plymouth, New Zealand, Thomas Avery and Sons, 1959. £2 2s. Essays presented to H. D. Skinner ranging from Jack Golson on 'Culture Change in Pre-historic New Zealand' to studies on modern Maori Communities.

ANCIENT INDONESIAN ART. By A. J. BERNET KEMPERS. pp. 124, 353 plates, 9 text figures and maps. Cambridge, Massachusetts, the Harvard University Press, 1959 (London, Oxford University Press). £9.

THE EXCAVATIONS AT DURA-EUROPOS. Final Report V, Part I. pp. 457, 71 plates, New Haven, Yale University Press (London, Oxford University Press), 1959. £12. This volume deals with the Parchments and Papyri and is written by C. BRADFORD WELLES, ROBERT O. FINK and J. FRANK GILLIAM.

INDIAN TEMPLES AND SCULPTURE. By LOUIS FRÉDÉRIC. pp. 464, including 424 photogravure illustrations. London, Thames and Hudson, 1960 (dated 1959). £5 5s.

[continued on p. 110.]

Human Evolution and Neanderthal Man

by B. ŠKERLJ

The Professor of Anthropology in the University of Ljubljana, in Yugoslavia, examines, in the light of recent discoveries, the various theories that have been put forward with regard to the relationships of Homo neanderthalensis to Homo sapiens, and refers briefly to the problem of Oreopithecus, already discussed by Dr J. C. Trevor in these pages (ANTIQUITY 1959, 131). He argues that the Neanderthals certainly belong more to the Pithecanthropi than to modern man and offers us a valuable new system of classification and nomenclature.

TO celebrate the 100th anniversary of the Neanderthal discovery, I published in 1956 a paper on the 'Role and Position of Neanderthal Man in Human Evolution'. There I stated, 'Although the question of man's natural origin from a Primate form is basically settled, there is still much controversy about the mode of human evolution from that (hypothetical) ancient Primate.' Four years later this statement still holds, although new facts and views have thrown some fresh light on the evolutionary problems of *Homo sapiens*. Nevertheless, in spite of the constantly improving palaeontological inventory available to us, and all our knowledge of evolutionary factors gained in the century since Darwin's *Origin of Species*, the data are still too meagre to allow only one interpretation of human evolution.

After having read Le Gros Clark's valuable paper on 'Bones of Contention' (1958) I was glad to find him, on many questions, in agreement with my views expressed in 1956, as will be clear to some extent from the present article. On the other hand, there are points of disagreement as well, the foremost being the evaluation of the Palestine finds. After the thorough study of this material by Thoma (1957-8), I am even more convinced than ever that the Palestinians represent a hybrid population between *H. neanderthalensis* and *H. sapiens*, as already suggested by Coon in 1939 and by Montagu in 1940.

New light has been thrown on man's evolution by Hürzeler's discovery of an almost complete skeleton of *Oreopithecus* at Grosseto (Tuscany, Italy) in August 1958. Although we have no full scientific account of this find as yet, just about the time when the skeleton was unearthed, Hürzeler summarized all our knowledge on *Oreopithecus* since its first discovery in 1869. According to this account, *Oreopithecus* apparently shows quite numerous traits suggestive of the human line of Primate evolution, at a time in the early Pliocene which fits our ideas about the antiquity of man (in the sense that includes all forms of the family *Hominidae*).

Whereas, at present, we have no final report on *Oreopithecus*, and do not know whether he was able to walk only, or preferably, on his hind limbs, it is commonly agreed that the *Australopithecinae* had acquired an upright gait. This could have happened towards the end of the Pliocene period, if not earlier. They were also the first known Primates who, very probably, fed on appreciable amounts of animal proteins and had probably acquired some hunting habits. Other primates, however, fed only partly on insects and eggs. Devoid of any bodily means of defence or aggression they became dependent on some kinds of implements, as was convincingly shown by Bartholomew and Birdsell (1953). The tran-

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sition from a purely gathering economy to a hunting one is, beyond question, of decisive importance in the evolution of man's ancestry. However, the only Australopithecines yet known are geologically too young to be considered direct forebears of man. Therefore, the Pliocene layers, somewhere between Kenya and South Africa, may be of the greatest interest to all students of human evolution.*

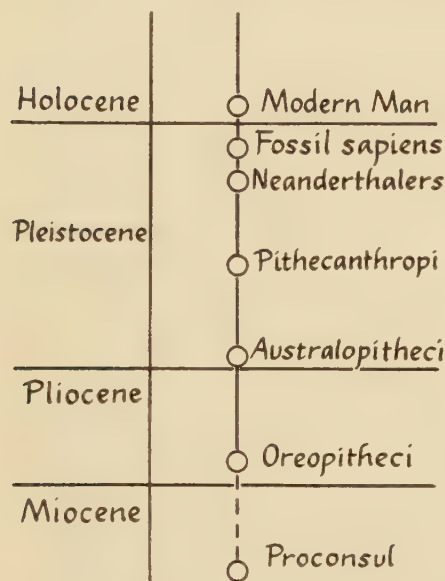


Fig. 1. The unilinear theory of evolution in Man in its simplest form. Nevertheless, according to Wanke (1955), it can be proved even by mathematical methods (reference points) from *Pithecanthropus erectus* up to the modern Australian aborigines through the various *Neanderthals* and *Fossil Modern Men*.

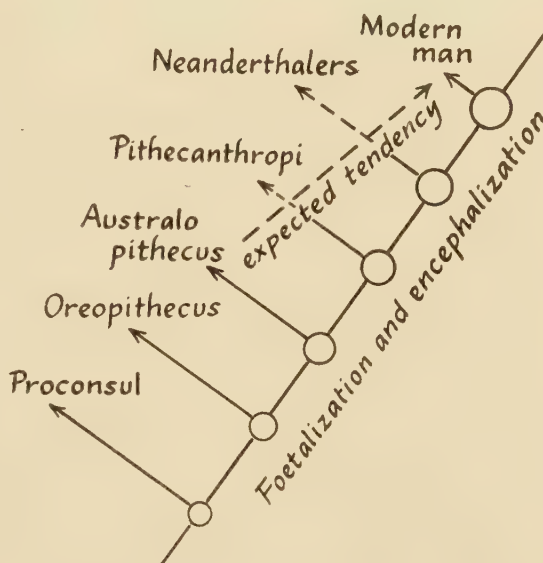


Fig. 2. The ever-shortening arrows represent the gerontomorphization factor as it should appear according to the geological segments of 'types' in a unilinear and stadial evolution. The difficulty, however, is that the *Pithecanthropi* and, at least the 'classic' *Neanderthals* developed heavier brow-ridges than had their hypothetical forebears, the *Australopithecines* (or at least most of them) and even *Proconsul*.

Taking the position of the *Australopithecines* for granted, the difficulties with regard to the mode of man's evolution begin with the Pleistocene period, when bipedal Primates, acknowledged commonly as *Homininae*, began to roam the old-World continents almost simultaneously. There are some anthropologists who insist upon a stadial unilinear evolution of man, as offered clearly by Stolyhwo (1908) and Hrdlička (1927), considering *Neanderthal man*—then recognized in one 'classical' European form—as an evolutionary stage of modern man. Logically, these authors consider *Pithecanthropus* to be a still earlier evolutionary stage and, later, *Australopithecus* was added, particularly by Russian authors, as the first known human stage on this unique evolutionary line. Very probably, should *Oreopithecus* be recognized as a biped, he will be placed prior to the *Australopithecinae* (cf.

* Meanwhile, in July, 1959, *Zinjanthropus boisei* was discovered by L. S. B. Leakey and his wife just in the northern part of the region mentioned above. This discovery yields the proof that the *Australopithecines* were the first toolmakers and hunters known so far. (Cf. *Current Anthropol.* 1: 76-77, 1960.)

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FIG. 1). It is important to be aware that unilinear evolution and monophyletic origin are not synonymous. As will become clear later, a monophyletic origin of the *Hominidae* from a Primate stock does not logically imply unilinearity of evolution.

The unilinear hypothesis of evolutionary stages implies an orthogenetic evolution or, at least, the danger of such a background, including a supernatural, teleological force. Biologists consider orthogenetic evolution as exceptional, and there is no *a priori* reason to accept such an hypothesis for the evolution of man. In the form as offered by Hrdlička and some modern Russian, Polish, Czech, and other anthropologists (cf. Nesturkh, 1958; Roginsky and Levin, 1955; Wierciński, 1956; Malá *et al.*, 1956) it has little to do with Darwinistic evolution based on natural selection, isolation and chance mutation. Admittedly, however, such a unilinear hypothesis made some sense some ninety to thirty years ago, when evolution in man had to be presented convincingly to the still reluctant majority of laymen, and when the scarce palaeontological inventory of human bones seemed to support such an exceptional mode of evolution. Such a simple unilinear hypothesis seems somewhat suspect after the discovery of the *Australopithecinae* with their relatively undeveloped browridges (as compared to *Pithecanthropus* and Neanderthal man). This hypothesis is even more unlikely after finding some relatively modern-looking human remains from the Middle Pleistocene, almost contemporaneous with *Pithecanthropus*. Besides, to compare only adult forms from a purely anatomical point of view does not cover many other important biological questions. Though many anthropologists—even Hrdlička himself—had an adequate anatomical (and even medical) training, they lacked biological erudition and thinking. To avoid difficulties for the unilinear hypothesis the simple recipe, first invented by Weinert (prior to World War II), and still practised by some authors, is not to recognize or even mention such finds as Kanam, Kanjera, Swanscombe or Fontéchevade. Other authors declare Fontéchevade as a Neanderthal man with traits of the *sapiens* form—which is to my mind only playing with words.

There is, however, the need to take a stand as to the finds just mentioned. I shall do it very briefly. According to K. P. Oakley (1958)¹ Kanam does not seem quite so certainly lower Pleistocene as it appeared more than twenty years ago to L. S. B. Leakey and the Commission who judged it together with the Kanjera remains. Although its antiquity and morphology are still sub-judice, the Kanam mandible is highly mineralized and, also according to Montagu (1957), the *impressio sub-incisiva* is clearly developed. Kanjera, on the other hand, may well be of Middle Pleistocene age, also according to the latest investigations. These finds, as I know them, are definitely neither Pithecanthropoid nor Neanderthalian. Swanscombe, according to Breiting (1955), might have had a forehead with browridges, although its vault is high and the occipital bone is not Neanderthal-like. This may place Swanscombe, which is definitely of Middle Pleistocene age, somewhere in the neighbourhood of the still enigmatic Steinheim skull. Fontéchevade from the beginning of the Last Interglacial is definitely of modern form despite such wary suggestions as 'Neanderthal with sapiens forms and traits', as offered by some of the above-named authors who never had the specimens in their hands. That the two Fontéchevade skulls belonged to any Neanderthal form is, according to the thorough monograph by Vallois (1958), out of the question. Nevertheless this 'modern' man lived, in France, apparently prior to the Neanderthals.

The 'problem' of Palestine man seemed to me settled by Coon's hypothesis of hybridization, advanced in 1939; since then, this interpretation has been supported not only by the Fontéchevade finds but also, most cleverly, by the hybridization study carried out by Thoma (1957-8). A still open problem is that posed by Steinheim: is it 'Neanderthal with

¹ Acknowledging with thanks also private information from the same author.

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sapiens traits' or, vice versa, i.e. an ancient sapiens form with Neanderthal characters? Or is it a transitional form between a Middle Pleistocene (?) Neanderthal or Pithecanthropoid and sapiens? Or is it a hybrid form between—well, what?

Furthermore, those 'quasi-Neanderthals', as well as the 'pre-sapiens' specimens, allow for more questions; e.g. where did the 'classic' Neanderthals come from? Dating Teshik-Tash as Mindel-Riss does not seem justified by the evidence (Škerlj, 1951; Movius, 1953). Had they developed outside Europe or were they genuine descendants of a Steinheim form of man? This does not seem impossible for the western 'classic' Neanderthals. On the other hand, if they developed from Heidelberg man, should not there be expected more finds in the long Mindel-Riss period? Particularly one might expect at least some find of a less 'progressive' type than Steinheim. Why and how did the Neanderthals disappear at the end of the first Würm stadial? Were they wiped out or were they amalgamated with a more successful form of man?

As to the last question, amalgamation of Neanderthals by hybridization with 'sapiens' contemporaries seems to me one of the reasons for their disappearance. Another reason: according to F. C. Howell (1952) Neanderthals were well adapted to the cold. Despite his thorough study I cannot agree with this conclusion. As for 'classic' Neanderthals, their skeletal remains do not seem to fit either Allen's or Bergmann's rules. The 'classic' Neanderthals were relatively short, had a relatively big head with prominent brow-ridges and noses, relatively big hands and feet, all with relatively large surface areas in their distal parts. Besides, I doubt that they already had invented an Eskimo parka. Therefore, comparing them to the modern Tungus or Eskimo, they may be considered as not well adapted to cold climates. After all, at that time (Würm I), 'classic' Neanderthals were very probably too specialized physically and culturally to adapt successfully to the climatic stresses of a glacial period.

Considering the foetalization theory, according to De Beer (1948) the Neanderthals might be the real ancestors of modern man. Beyond doubt the foetalization theory has a sound core; yet when comparing some relatively primitive characters of the *Australopithecinae* and modern *Homininae* on the one hand, with some apparently specialized features (such as the highly protruding browridges) of the *Pithecanthropus* and, at least, 'classic' Neanderthal man on the other, difficulties arise. As shown in FIG. 2, where the interaction of foetalization and gerontomorphization² is shown schematically, even the foetalization theory does not seem adequate to overcome the difficulties posed by Pithecanthropoids and 'classic' Neanderthals.

Therefore another hypothesis of evolution, which duly considers foetalization together with encephalization, as well as geronto(thero)morphization, is presented for further discussion in FIG. 3. At this moment I do not wish to anticipate questions of taxonomy and nomenclature, because all the characters and forms under discussion may appear in a polymorphic species as well as in a genus with different species, or even in higher systematic categories.

The problems of the taxonomy, systematics and nomenclature of Man are among the most disputed in anthropology. Almost any author, including the present one, puts forward his own point of view, some with good reasons. Taxonomists in zoology, as well as geneticists do not—and cannot—feel content that almost every new hominid find gets a new generic name—the last one being *Zinjanthropus* (1959),³ and before that *Atlantropus* (1953).

With the *Australopithecines* their systematic range seems to become clear, most anthropologists consider them as a subfamily (*Australopithecinae*) of the family *Hominidae*.

² Gerontomorphism, in man, equals or almost equals theromorphism.

³ The news of this discovery came after the present paper had been prepared for press.



Fig. 3. The evolution of Man may have gone this way, whereby in one line gerontomorphization, in the other foetalization, prevailed. There is no need to split mankind into many genera (and perhaps not even species) at any known evolutionary phase, and crossings (hybridization) may have occurred on any evolutionary level between races and sub-species (or even between species).

Most competent authorities agree also in dividing them in two genera, viz. *Australopithecus* and *Paranthropus*, some (Le Gros Clark, 1955, and Riesenfeld, 1955) recognize only one genus—*Australopithecus*. To me, *Paranthropus* seems to deserve separate generic status, for although its gait was very probably bipedal, qualifying it for inclusion among the *Australopithecinae*, a sagittal crest and a more or less Gorilla-like *torus* (with a depression behind it)⁴ distinguishes it from the more human-like genus *Australopithecus*.⁵ There are, according to Robinson (1954), more species and, perhaps, subspecies in both genera of the *Australo-*

⁴ I had the opportunity to examine a cast, M-16922, in the British Museum (Nat. Hist.).

⁵ Unfortunately, however, according to the literal meaning of its generic name *Australopithecus* should be more ape-like than *Paranthropus*, whereas just the opposite seems to be the case.

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pithecinae. Doubtless, however, taxonomic and systematic problems remain to be cleared also with the Australopithecines, and new finds as well as laboratory work will provide for more decisive answers; this applies also to the position of the so-called South-Asian Australopithecines. As for *Telanthropus*; one mandible is probably inadequate for a definite taxonomy, but it does not appear to belong among the Australopithecines. The mandible is more reminiscent of mandibles of the *Pithecanthropi* than of any of those among the proven Australopithecines. Such a mandible could be easily connected with a more spacious braincase than is usual among that group.⁶

More and more competent anthropologists, with the full assistance of zoologists and biologists, agree that the baptizing of every new find with a new generic name was—and still is—an abuse. It is, however, legitimate and convenient to recognize in the family *Hominidae* bipeds with small brains (the subfamily *Australopithecinae*) as well as those with big or 'typically human' brains (the subfamily *Homininae*).⁷ But, how many genera and species should be recognized among the latter? Most of the pertinent literature gives the impression that there must have been many genera and species, for almost every new find carries according to the happy discoverer a new generic, and certainly a new specific name. Though this approach to new finds is humanly understandable, it cannot convince a dispassionate critic. Let us here consider only three criteria which seem to be rather more important than others: (a) morphology, (b) brain size, and (c) hybridization.

(a) Considering the morphology of the skull, two clearly distinct types appear: a gerontomorphic group comprising *Pithecanthropi s.s.* and *Neanderthal man*, and a paedomorphic one including all human beings with a smooth and relatively high braincase without browridges, but with a chin.

(b) As to brain volume, two distinct groups may be discerned, although this grouping does not correspond with that arrived at under (a). Thus one of the brain-size groups comprises *Pithecanthropi s.s.* only, while the other brackets *Neanderthals* and *Modern man*—morphologically different though they be.

(c) Considering hybridization, the main question remains, whether crossings between all the 'types' named under (a) and (b) could have been not only possible but also fertile. In agreement with Coon (1939), Montagu (1940, 1951) and some other authors including Thoma (1957-8), I am convinced that such crossings were possible and that they happened relatively often. A necessary condition may be, of course, that various forms of the *Homininae* emerged approximately at the same time out of an adaptive radiation centre (cf. Simpson, 1951), probably towards the end of the Pliocene period somewhere in the African savannah. According to the palaeontological record this condition seems to be fulfilled. There are some positive signs of hybridization, at least towards the end of the Pleistocene period (Palestine, Předmost), while the evidence of Steinheim and, perhaps, Swanscombe may be indications of the occurrence of fertile hybridization still farther back.⁸ If we take the possibility of fertile hybridization for granted, the *Homininae* cannot be divided into several genera; therefore they must comprise only one genus: *Homo*. That this single genus

⁶ It seems that all generic names of the *Australopithecinae* taken literally are misnomers, but according to the rules of zoological nomenclature they cannot be changed and will have to continue to serve as labels.

⁷ The position of *Oreopithecus* is still undecided, perhaps eventually we shall have to recognize a third subfamily—*Oreopithecinae*.

⁸ It is very probable that we would not speak of hybridization between taxonomic categories if we could not perceive differences between two 'types' which were in evolution. As long as they are very close we are not able to perceive and judge their genetic basis and evolutionary meaning, although this may become possible at a later stage of their evolution. Interbreeding goes on in any evolving form till the stage of two (or more) 'good' species is reached. Speaking of hybridization here, we mean interbreeding between two well-distinct taxonomic categories at least on the level of races and subspecies.

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developed—at the beginning of the Pleistocene period—in divergent directions, is shown by the palaeontological finds. This genus was splitting into local adaptive types and diverging towards more species some of which, perhaps, reached even the category of subgenera. Nevertheless, they all may have been capable of intermingling (partly, maybe, in a non-fertile manner like horses and asses).

Considering all this we have to return to the criteria (a) and (b). Logically, divers 'genera' like *Pithecanthropus*, *Sinanthropus*, *Africanthropus* and *Atlanthropus* have to disappear as such completely; many anthropologists recognize this already. In connection with our answering the criterion (c), the above 'genera' may be considered as belonging, at best, to only one subgenus, viz. *Homo* (*Pithecanthropus*), or as belonging to one species of the genus *Homo*, viz. *Homo erectus* (with more subspecies: *erectus*, *pekinensis*, *heidelbergensis*, *mauritanicus*.)

The question of the position of the Neanderthals, however, is not settled yet. According to the criterion (a) they should belong to the subgenus *Pithecanthropus* (or the species *Homo erectus*), whereas according to (b) they belong to the subgenus *Homo* (or even to the species *Homo sapiens*, but as a separate subspecies). Although the brain volume in primate evolution constitutes an important taxonomic criterion, it cannot be as decisive in human evolution. In living man at least, the variability of this volume is exceedingly large (between approximately 850 and 2000 c.c.) yet being apparently without any meaning in terms of living or selective advantage.⁹ Therefore the finer anatomy and structure of this organ must be taken into consideration. From the gross-anatomical point of view the brain of the Neanderthals seems to have been different from that of Modern man, though there is no considerable difference as to volume; it was only differently distributed in the braincase: in Neanderthal man the frontal and parietal lobes were relatively smaller than in Modern man, whereas the occipital and temporal lobes seem to have been relatively larger.¹⁰

The volume of the Neanderthal brain can be estimated on the basis of the data of ten individuals (cf. Bunak, 1941; Count, 1947; v. Eickstedt, 1938; Saller, 1930): converted into mass it must have been in the neighbourhood of 1340 gm. on an average. Estimating the body mass of the Neanderthals in accordance to their stature (about 160 cm.) and muscularity at 60 kg., it becomes apparent that 1 gm. of brain mass corresponds to approximately 45 gm. of body mass (it may correspond to about 37.7 gm. of lean body mass, though this is probably too low an estimation). In Modern man the average brain mass (according to the series enumerated by R. Martin, 1928) is 1280 gm., whereas stature (for both sexes all over the world) is about 163 cm., and body mass 58 kg. So, 1 gm. of brain corresponds to 45.4 gm. of body mass, or—on a low estimate—39.7 gm. of lean body mass.¹¹ Although the Neanderthals might have had a somewhat larger brain mass, the relative encephalization in both types seems to be practically the same. This might be considered also as the proof that a large brain mass *per se* is not and—as the end of the Neanderthals in the Würm Glaciation proves—never was under the protection of natural selection. This generalization may only apply after a certain minimum of brain size had been reached. On the other hand, I cannot see why Neanderthal man should have had the same psychological potential as Modern man, as Montagu (1951) suggested.

⁹ This huge 'meaningless reserve' of brain was a main point of the discussion between A. R. Wallace and Darwin on the role of natural selection as to brain development in man (Eiseley, 1955, 1959).

¹⁰ According to Weil (cf. Matiegka, 1937) the surface area of the part of the brain in front of the vertical line touching the frontal edge of the temporal lobe was in Neanderthals 21–25 per cent only, whereas in Modern man it is 28–31 per cent of the whole brain surface area.

¹¹ It may well be that this quotient is inappropriate if one considers *fossil* Modern man, with estimated average brain mass of 1350 gm. (based on 16 individuals) and approximately 65 kg. of body mass of this rather heavily built type of man.

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subspecies *Homo erectus neanderthalensis* also various races are to be distinguished, and the same is the case in the subspecies *Homo sapiens sapiens*. If one wishes to distinguish (as Linnaeus and Darwin did) the Negroes, Whites and Yellow peoples (Mongoloids) as subspecies, this may become a further item of discussion. A greater obstacle involves the systematic position of the Australian aborigines, African Bushmen and Hottentots, various Pygmies—all of whom should be considered as subspecies if 'the big three' rank as subspecies.¹³ Taxonomy cannot depend on numerosity only, it has to consider—logically—real characters of distinction. My own view is that the three main groups of living men are not in the range of subspecies; they may be considered as races within the subspecies *Homo sapiens sapiens*. The same holds, of course, also for the above enumerated smaller groups of living mankind.

To end this paper, the author wishes to stress the very problematic taxonomy and position of the Neanderthals who, as a whole group, morphologically stand nearer to the *Pithecanthropi* s.s. than to *Homo sapiens*. A central problem concerns the finds of Steinheim and Swanscombe, whereas Fontéchevade now (since the appearance of Vallois' monograph) seems fully settled. That the Neanderthals are at least subspecifically different from *Homo sapiens*, and that some Pleistocene finds, partly at least in Palestine and Předmost, represent the results of at least subspecific intermingling, is the firm conviction of the present author so far. To consider these specimens as evolutionary transgressional seems wholly unjustified from various biological, evolutionary, and particularly genetical reasons. Furthermore, in comparison with my views expressed in 1956, this paper comes to a more radical conclusion, eliminating the then still acknowledged subgenera, and stressing the probability of hybridization between Modern man and Neanderthals whatever both these terms comprise. Even an hybridization on a specific level in an earlier evolutionary stage seems not impossible, though perhaps in a non-fertile manner.

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¹³ This seems to be also in accordance with Coon's views (1959).

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Socotra

by P. L. SHINNIE

Professor P. L. Shinnie, who now occupies the Chair of Archaeology in the University College of Ghana at Accra, here discusses the results of an archaeological reconnaissance made in 1956 in the island of Socotra.

THE island of Socotra lying on the direct sea route from the Red Sea to India has long been thought of as a fruitful field for archaeological investigation and as likely to provide information on ancient and medieval trade in the Indian Ocean. Its inaccessibility,¹ (there is no regular means of travel to the island and it can only be reached at certain times of year by dhow from Mukalla and the Persian Gulf) has resulted in its being little visited in recent times. The only previous visitor to the island whose interest was primarily archaeological was T. J. Bent who visited it in 1897 and whose results, of a somewhat sketchy character, were published posthumously by his wife in the book *Southern Arabia*. Since the latter part of the last century, when there were several expeditions,² few, other than administrative officers from the Aden Protectorates, seem to have visited the island.

In 1956 an Oxford University party, which I was able to accompany, went there and it was possible in the two months available to carry out some survey of the antiquities.³

As will be apparent from this account only a portion of the island was explored. This was in the main due to the extremely rough nature of the country which made it necessary for all journeys to be made on foot, with donkeys as baggage bearers. As a result of this, movement was slow and restricted, and in one case a journey had to be abandoned and a return made to base earlier than had been planned owing to exhaustion of food supplies, lack of water, and the absolute impossibility in this poverty stricken land of living off local supplies which scarcely suffice for the needs of the Socotrans themselves.

Botting has given a description of the island and a sketch of its history, but something should be said here of both to show the setting in which the archaeological material was found.

Socotra is about 72 miles long from east to west and about 22 miles across at its widest point (FIG. 1). It consists of a central mountain range rising to a height of over 4600 feet in the Haggier massif and running out eastwards to Ras Momi and westwards to a broad

¹ It is 220 miles from the south coast of Arabia and 150 miles east of Cape Guardafui at 12° 30' N. and 54° E.

² Bent's already referred to, a botanical expedition by Balfour in 1880, the German expedition of Riebeck in 1881 in which Schweinfurth took part, the Vienna Academy of Science expedition of 1899, and the predominantly zoological one of Forbes in the same year.

³ A general account of the expedition has been published as *The Island of Dragon's Blood* by Douglas Botting who planned and led the expedition. Acknowledgements to those whose support made the trip possible are given in *The Oxford Exploration Club Bulletin*, No. 10, but I must here express my thanks to those whose contributions were specifically for archaeological purposes: The Society of Antiquaries, the British Academy, The Trustees of the British Museum, The Seven Pillars of Wisdom Trust, and the Ghana National Museum.

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plateau above the town of Kallansiya. Settled population is largely restricted to the northern coastal plain which is divided into three main areas by outliers of the mountain range which run down to the sea. At the west end is the comparatively fertile plain of Kallansiya (not visited by the expedition); further east is the plain of Eriosh, a wide flat area on which a war-time aerodrome was built; and then further east again and separated from Eriosh by an extremely difficult route across the hills, is the plain on which stands Hadibo,⁴ the capital, as well as the village of Suk which appears to have been the main town until some time in



Fig. 1. Sketch map of the island of Socotra.

the 18th century. This plain has two rivers which flow strongly during the rains and has several fresh water lagoons. It is here that not only the political centre of the island is based but also most of such little prosperity as there is. The residences of the Sultan and his wazir are here, and there is also a small colony of foreigners, mostly Arabs from the coast of south Arabia and the Persian Gulf. From Hadibo and Kallansiya are carried out such simple mercantile activities as the meagre products of the island inspire.

The southern coastal plain, much less well watered, is far poorer and its small population is restricted to two or three villages of extreme desolation and poverty. Like the equally poor inhabitants of one small community at Ras Momi at the extreme east end of the island, they exist largely on shell fish and the areas around their villages form considerable shell middens.

Whatever may have been the earlier history of Socotra, and speculation as to its being the island of the Shipwrecked Sailor in the Ancient Egyptian story has as yet no fact to support it, it was certainly known to the Romans and is mentioned amongst others by Pliny.⁵

The earliest description is that given in the *Periplus of the Erythraean Sea* where the author says 'there is an island . . . it is called Dioscorida, and is very large but desert and marshy, having rivers in it and crocodiles and many snakes and great lizards of which the flesh is eaten and the fat melted and used instead of olive oil. The island yields no fruit, neither vine nor grain. The inhabitants are few and they live on the coast towards the north, which from this side faces the continent. They are foreigners, a mixture of Arabs and Indians and Greeks, who have emigrated to carry on trade there. The island produces

⁴ Shown on some maps by the name of Tamarida.

⁵ *Nat. Hist.*, vi, 153.

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the true sea-tortoise, and the land-tortoise. . . . There is also produced in this island cinnabar, that called Indian, which is collected in drops from the trees.

It happens that just as Azania is subject to Charibael and the chief of Maphritis, this island is subject to the king of the Frankincense country. Trade is also carried on there by some people from Muza and by those who chance to call there on the voyage from Damirica and Barygaza; they bring in rice and wheat and Indian cloth, and a few female slaves; and they take for their exchange cargoes a great quantity of tortoise shell. Now the island is farmed out under the Kings and is garrisoned.⁶

This description is not very informative and it is not certain that it is a first-hand account. Certainly the crocodiles, lizards, and tortoises are not found now, though presumably they may have died out since the 1st century A.D. What is important is that the author correctly noted that the population was on the north side of the island, that it was subject to the king of the Frankincense country which must be the coast of South Arabia,⁷ and that it produced Cinnabar. This cinnabar, or 'dragon's blood' is the exudation of the tree *Dracaena cinnabari* which grows commonly on the island and was until recent times an article of trade.⁸ From other early writers it is clear that aloes and incense were traded, and both aloes and incense trees occur abundantly now though they are no longer used in commerce.

Of the later history of the island little is known until the arrival of the Portugese but it is referred to by several medieval Arab writers,⁹ to whom is due the story that the inhabitants were Christian. This story is also confirmed by Marco Polo and a 17th-century Carmelite monk.¹⁰

The Portugese first discovered the island in 1503 and in April 1507 under the command of Tristao da Cunha and Alfonso Albuquerque they landed and attacked the fort at Çoco (Suk). A vivid account of this battle is given from which it is clear that the island was then under the control of Mahri Arabs, whoever the Christian inhabitants may have been.¹¹

The fort was repaired and occupied by a Portugese garrison and the nearby mosque was turned into a Church of 'Our Lady of the Victory'. The life of the garrison was not a happy one, supplies were difficult to get, and 'Christian' natives seemed more disposed to ally themselves with the Arabs than with the Portugese. In 1511 the fort was finally abandoned and the Mahra re-established their rule.

Various European seamen called at Socotra during the following centuries, including Sir Thomas Roe of the East India Company who assumed the inhabitants to be Christians because one of his seamen ' . . . saw an old church of theirs in the way to Tamara, left desolate, the doore shut, but onely tyed . . . he adventured in and found an Altar with Images and a crosse upon it.'¹²

In 1801 the Wahabis invaded the island but do not seem to have seriously affected its dependence on the Sultan of Qishn. The East India Company established a coaling station here in 1835 but this only lasted until 1838, when a move was made to Aden. In 1886 Socotra became a British Protectorate as a dependency of Aden.

The archaeological survey was at first centred on the Hadibo plain, since it seemed likely, both from the Periplus description and from the modern distribution of cultivation and population, that here would be found any traces that might exist of contact with the classical

⁶ Schoff's edition, 33, 34.

⁷ Socotra now forms part of the Sultanate of Qishn and Socotra. Qishn being the capital of the Mahra country on the mainland of the eastern Aden Protectorate. This link seems a long-standing one.

⁸ It is used as a pigment.

⁹ Amongst these may be mentioned el Hamdani, Yakut, and Idrisi.

¹⁰ Bent, *Southern Arabia*, 355.

¹¹ *The Commentaries of the Great Afonso Dalboquerque*. Hakluyt Society, vol. LIII, 1875, 44-56.

¹² *Purchas his Pilgrims*, Pub. Maclehose, IV, 321.

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and earlier world. This plain is only 7 miles long by 3 miles wide and it is therefore possible to examine it in considerable detail, and from various vantage points the whole plain can be viewed.

In spite of the most diligent search no trace of any occupation of certainly earlier date than the late Middle Ages was found. In the hills there are many examples of early agriculture in the shape of terraces and field boundaries, and some of these may date from the days of the early incense trade. It is significant that the stone walls and terraces occur only at an altitude which is suitable for the growth of incense trees and there can be little doubt that this is what they were for; but there is no evidence whatsoever as to date except that they are in the main not modern and most of them are not now in use. It is likely that the lagoons along the shore of the Hadibo plain were used by early seamen—certainly by the Portugese—and they would have been very suitable for the small boats of ancient and medieval trade.

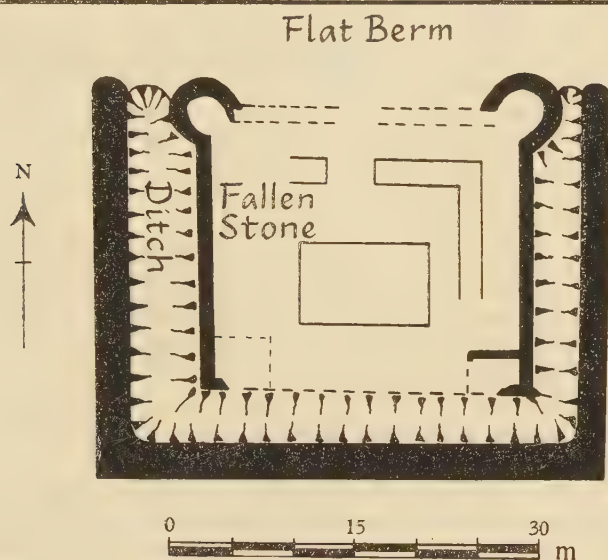


Fig. 2. Plan of Hasun fort.

The following sites were examined in this area:

Hasun. On a small conical hill about two miles inland and south-east of Hadibo, and to the east of the main wadi that runs down from the hills to the sea, is a small fort. This is presumably the fort referred to by Sir Thomas Roe in 1615 when he said: 'There is a castle foure square on a hill, a mile from Tamara (= Tamarida), but he could not get license to see it.'¹³ It was also seen by Bent who says of it: 'On the plain behind Tamarida there is a conical hill about 200 ft. high called Hasan, which has been fortified as an Acropolis, and was provided with cemented tanks. These ruins have also been called Portugese, but they looked to us more Arabic in character.'¹⁴ It may well be that it was built or at least reused by the British during the occupation of the 1830s, but there is no direct evidence as to date.

¹³ Purchas his Pilgrims, loc. cit.

¹⁴ Bent, op. cit. 363.

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The fort is now much damaged and the walls tumbled. The inside is largely filled with blocks fallen from the walls so that the internal arrangement is largely obscured. It can, however, be seen that it was a rectangular building with ditch and counter-scarp on the three landward sides and a broad beam on the seaward side (FIG. 2). The main outer wall is built of undressed stone blocks set in mud mortar. There is a small central room about 8 by 5 m., built of smaller stones and set in good quality cement. This is no doubt one of the tanks referred to by Bent, and it may in fact have been a tank. Only removal of all the fallen and tumbled stone which now encumbers it can resolve this. The fort is in a splendid strategic position and dominates the whole of the Hadibo plain.

Sirihin. There are ruins of a village of this name halfway between Hadibo and Suk. There is no evidence as to its antiquity and local accounts are not helpful. Some of the ruined houses do not appear to be old, but there are also a number of walls now standing only a few inches high and built with good cement which must be of some antiquity.

Fort on Hadibo plain. Immediately east of the main wadi, and near to the first of the war-time air-strips, is a structure consisting of a low earth mound forming a triangle. The sides are 69 m. long and 5 m. wide and 15 cm. high. This structure, occasionally referred to by visitors as a Portugese fort, seems more likely to be a relic of the occupation by the British in the 1830s when Indian troops are known to have been stationed on the Hadibo plain.¹⁵

Suk. Now only a small village, Suk lies at the very east end of the Hadibo plain under Jebel Hawari. As the 'Çoco' of the Portuguese and the earlier capital, it was an obvious point for archaeological investigation and, although now no more than a few miserable houses, it was clear at first glance that among these houses and along the west edge of the lagoon, formed where the wadi runs into the sea, were a number of mounds which suggested early occupation. Many of the modern houses are themselves built on mounds and ancient, or rather medieval, Suk must have been considerably larger than the present day village.

The inhabitants of Suk were well aware that it had been of greater importance in the past and they pointed out the sites of the mosque and the fort. Many of these mounds, unencumbered by modern houses, are now in use as cemeteries so that choice of an area to dig was severely limited. Fortunately the sites of the mosque and fort had not been so used and it was possible to dig there.

Work was started on the mound which was said by the inhabitants to have been the mosque. Although insufficient clearing was done to establish a plan of the building as a whole, there can be little doubt that the local information is correct and that this was the mosque of Suk in late medieval times and possibly considerably later. It was presumably this building which the Portugese turned into a church (FIG. 3 and PLATE XII (a)).

Owing to lack of equipment, excavation had to be on a small and simple scale. There was no trained digger among the men, for digging operations on Socotra are restricted to the digging of graves, the little work required for the simple agricultural needs of the community being carried out by the women. Digging itself was done by means of a crowbar with a pointed end, known locally as a *musmar*, and used in a sitting posture. The digging was therefore somewhat inefficient and slow, but, in spite of their total inexperience and the use of this peculiar tool, some of the men became quite skilled by the end of the work. Even with a crowbar, the digging itself presented no overwhelming difficulty, but, as there are no shovels on the island, the removal of the earth excavated presented a problem; it was taken away by small boys in baskets in the usual Near Eastern way, but the baskets had to be filled by hand by the boys or by the use of the base of the stem of a date palm leaf.

Part of the west end of the mosque was cleared to floor level. The floor was well made of

¹⁵ Balfour says that the trenches were still visible when he was there in 1880. Balfour, *Botany of Socotra*. Transactions of the Royal Society of Edinburgh, vol. 31.

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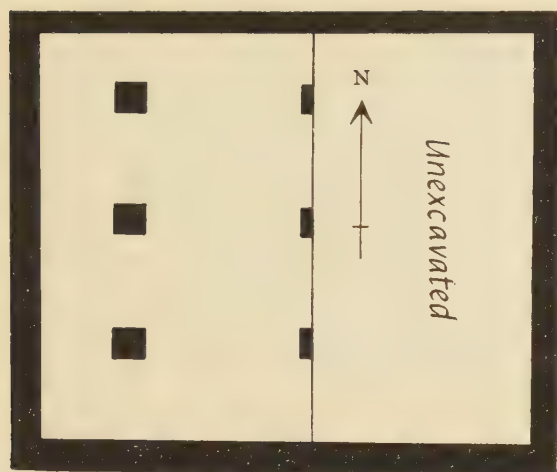


Fig. 3. Plan of Suk mosque.

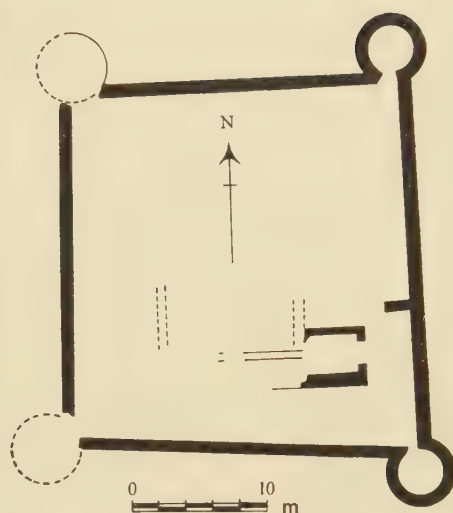


Fig. 4. Plan of Suk fort.

plaster. Six column bases of coral were found and it is reasonable to assume that there were nine columns in the building (FIG. 3). No material was found in the deposit on the floor.

A trench was dug into the mosque on its south side, through the spill of earth at the side of the mound, and up to the outer wall. Three distinct levels were found: level 1, of stony rubble; level 2, a dark earthy occupation deposit, with much ash, some glazed pottery, and much coarse ware; level 3, dark sandy earth, containing much sea shell; below this was natural sand. The pottery consisted largely of local wares, but some Chinese and Islamic glazed sherds were found. The Chinese ware was predominantly celadon. All the dateable pieces appear to be of the 16th or 17th centuries.

In the fort sufficient clearing was carried out for a plan to be made (FIG. 4). It consists of a main outer wall, of which only the lowest course remains, built of large undressed blocks of limestone and smaller blocks of coral, covered on the outside with a good hard white plaster. At each of the four corners was a circular tower or bastion. Traces of three of these, all badly damaged, can be seen (PLATE XII (b)). Except for the south-east corner, the whole building has been dug into and destroyed. Pits for burning lime have been dug into it and are in current use. Inner rooms and buildings have been so badly robbed and damaged that it is not now possible to see more than the vestiges of them. In the south-east part of the interior there is a room built in the same style as the main outer wall but of a rather smaller block. A part of this room was excavated, and the wall was found to go down to a depth of 2.4 m. from the top. From the west wall of this room, a much damaged wall runs at right angles for 5 m. and was presumably the wall of the main central building. Pottery found here, like that from the mosque, consists of unglazed local ware together with some Chinese and Islamic glazed pieces of the 16th or 17th centuries. In addition to pottery, 29 small iron balls, probably cannon balls, were found.

From Suk, a three-day march was made to Ras Momi, at the east end of the island, but no certain antiquities were found. Stone breastworks and some ruined houses east of Ras Dehammari are probably remains of occupation by allied troops during the last war, but

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in the absence of any local inhabitants from whom information could be obtained, there can be no certainty. These ruins did not appear to be old.

After the move of the expedition's base camp from Hadibo to Kishin in the hills, some investigation of the cultivation terraces, already referred to, was made. The northward slopes of the hills are in many places covered with these terraces and there were also many walls of dry stone construction which must have served as field and other boundaries. In some cases these walls run along the watershed for considerable distances and may have marked, as local information suggests, the frontiers between the territory of different tribal groups. Since the tops of the hills, where these walls occur, were in perpetual cloud, photography was a matter of extreme difficulty.

Local information also spoke of caves in the hills which had been used in former times for burials. Since all burial nowadays in the island are interments in the normal Muslim manner, any cave burials must have belonged to an earlier age.

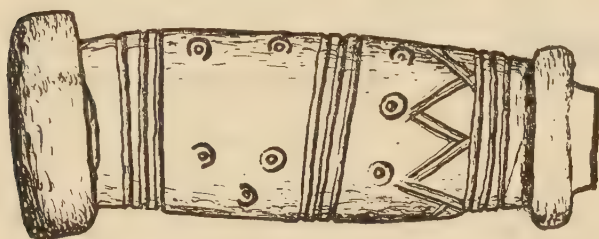


Fig. 5. Ivory object found with five burials in a cave near Kishin (scale 1/1).

One such cave, to which an informant took us, was visited in the hills near Kishin.¹⁶ The mouth of the cave had been blocked with a wall of dry stone construction, but much of this blocking had fallen inwards. In addition to the fall of stones, the depredations of animals had caused great confusion, but after the removal of much debris, it was possible to see that there had been five burials here. The only objects found were fragments of cloth, a small wooden vessel, and a peculiarly shaped ivory object (FIG. 5). No date can be given to these burials.

Dr Orr, in the course of his journeys in the mountains to obtain blood samples from the Bedu, came across a number of stone buildings described by the local inhabitants as being 'ancient' or 'European'. He describes them as follows:

1. On a col overlooking Kishin. A stone house with very large rooms. Main room, circular, 15-20 ft. diameter. Walls of roughly cemented stone. Four pillars of roughly cemented stones supporting radiating beams of acacia—beams radiate from pillar to pillar and pillar to wall, and support a roof 2-3 ft. thick composed of stones and earth.

2. At Mahadum, in the S.W. Haggier. Several 'European' houses, with walls of dressed stone and courtyards paved with flat stones.

3. On limestone plateau near Zerich. Old 'European' building. Floored entirely by natural limestone, well-built walls, in places double-thickness. One big courtyard about 10 yards long and three rooms in it—one room half filled and grassed over. Beyond that were two more courtyards at different levels, very ruined, and one had upright stones in it.

4. Several old houses at Adhein and under J.(ebel) Shelaley (PLATE XIII). Most of them built into the walls of a small field which one enters under a flat stone lintel. Piles of stones in the field and outside houses. Houses of varying size—one of them 30 ft.

¹⁶ A number of others in the hills further to the west were visited by Dr Orr who collected skeletal material from them.

long by 12 ft. wide. Present bedu houses have wooden beams—old houses have roofs supported on huge flat stones, some up to 6 ft. long, resting on a central pillar and radiating to the walls which sometimes slope in to meet them. One stone overhangs the other in a primitive attempt at vaulting. Round the walls raised beds. Doors often screened by small wall of stones. Small sheep pen adjoining. Used for cows nowadays.

5. On grass plateau above Molse under J.(ebel) Shelaley. A cascade of tiny walls and fields, walls often forming a parapet as well as a support. Village of two houses in a wall. In the middle of rectangular building 36 ft. by 12 ft., now just a line of stones divided into two rooms. One side of the wall incorporated a similar L-shaped building.

Ten yards further on a similar building 60 ft. by 12 ft. divided into square rooms.

Fifty yards further on remains of three buildings of different type. Double thickness walls of dressed stone, rectangular, one had had well-built fireplace of unusual dimensions built into the wall with a wide chimney outside. Wall no more than 5 ft. high but well made and in good condition. Two houses 30 ft. by 12 ft. and one house 9 ft. by 9 ft.'

An expedition was made from Kishin to the southern plain with the intention of visiting forts at Feragey and Ras Katanan.¹⁷ The party was unable to reach Ras Katanan owing to shortage of supplies and had to return to Kishin from the coastal village of Haif.

Feragey. The fort (PLATE XIV) which stands at the narrowest part of the valley that runs south from here to the coast, had already been seen and commented on by Bent,¹⁸ but no full description or illustrations have been published. It stands on the west side of the valley beside a pool. This appears to have been artificially enlarged and deepened and forms a type of moat, though running only along the front wall of the fort. The fort is built of red granite on a rock ledge which overlooks the pool. The main front wall is built of large boulders, the other walls of trimmed blocks, quite unlike the local building style with well-made accurate angles. The rear part of the building is much damaged and confused by modern goat pens and cultivation walls.

The fort is triangular in shape (FIG. 6) with towers at the two ends of the main front wall, which itself is only a retaining wall and does not rise above the ground level inside the fort. In the central courtyard is a well, and along the north-east side what appear to have been a series of rooms. There is no evidence for date, and no pottery or other material was found in the fort or its vicinity. Earlier writers have assumed that it was built by the Portuguese. I could see nothing to confirm this and I think it far more likely that it was built by Mahra Arabs to control the Socotri Beduin—perhaps when they re-established control in the 16th century after the departure of the Portuguese.¹⁹

The final expedition was to the Eriosh plain where, Wellsted, Bent, and others had recorded inscriptions on the rock, and ruins which Bent described as churches.

A forced march reached the inscriptions in a day and a half. These are cut in the flat limestone of the Eriosh plain and consist in the main of signs that are probably camel brands as well as the common emblem of a foot. A few may possibly be inscriptions (see FIG. 7).²⁰

Scattered over the plain are a number of rough stone ruins and it is probably these that

¹⁷ Described by F. M. Hunter, *Journal Royal Anthropological Institute*, VII, 371.

¹⁸ Bent. *op cit.*, 384.

¹⁹ Professor R. B. Serjeant in a personal communication tells me that from the photograph it looks to him much like forts he has seen in South Arabia.

²⁰ Some were copied by Hunter, *loc. cit.*, and by Riebeck and Schweinfurth (Glaser, *Skizze*, II, 184) but were apparently never published.

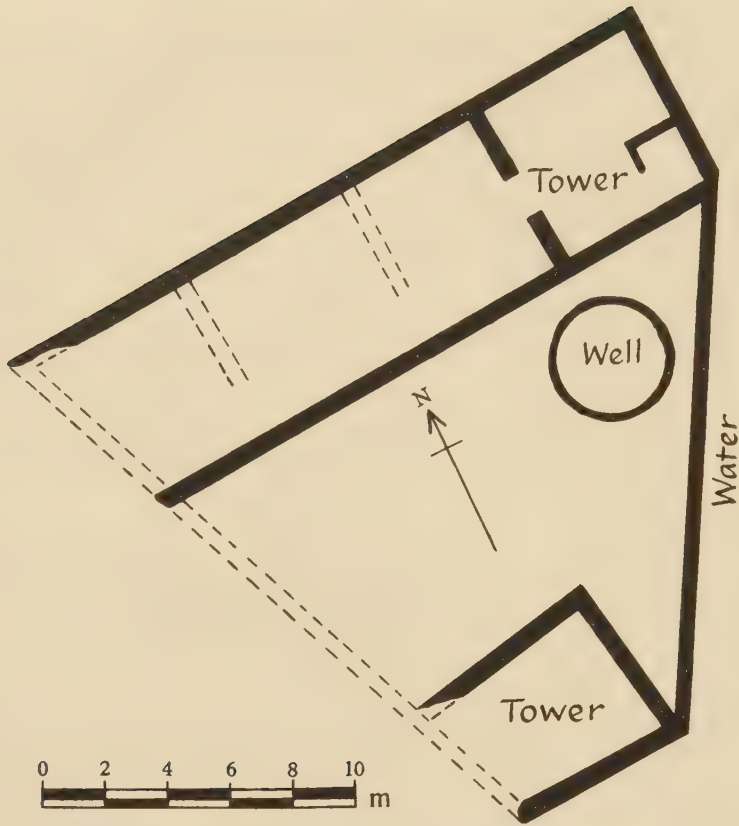


Fig. 6. Plan of Feragey fort.

Bent assumed to be churches. A careful examination makes it appear more probable that they are ruined huts of no great antiquity.

Lack of time prevented any examination of the western part of the island where other antiquities are said to have been seen by visitors. There is talk of a 'pagan temple' in the hills²¹ and Bent saw an inscription not far from Kallansiya.²²

The results of the survey of Socotra were not spectacular. Expectation of finding remains of classical antiquity were not fulfilled and, though it is not impossible that further investigation might find them, it will not be on the bare limestone plains of the north, where there is virtually no accumulation of soil. It must be assumed that those early traders, from whom the author of the *Periplus* got his information, spent little time on the island. We can well imagine that they came, as do the modern traders from South Arabia and India, for a few days only at a time, perhaps pulled their boats up on the beach, replenished their supplies of food and water, and passed on to India or up the Red Sea leaving little behind them.

[Appendix : see p. 110]

²¹ It is marked on Balfour's map.

²² Bent, *op. cit.*, Appendix IV.

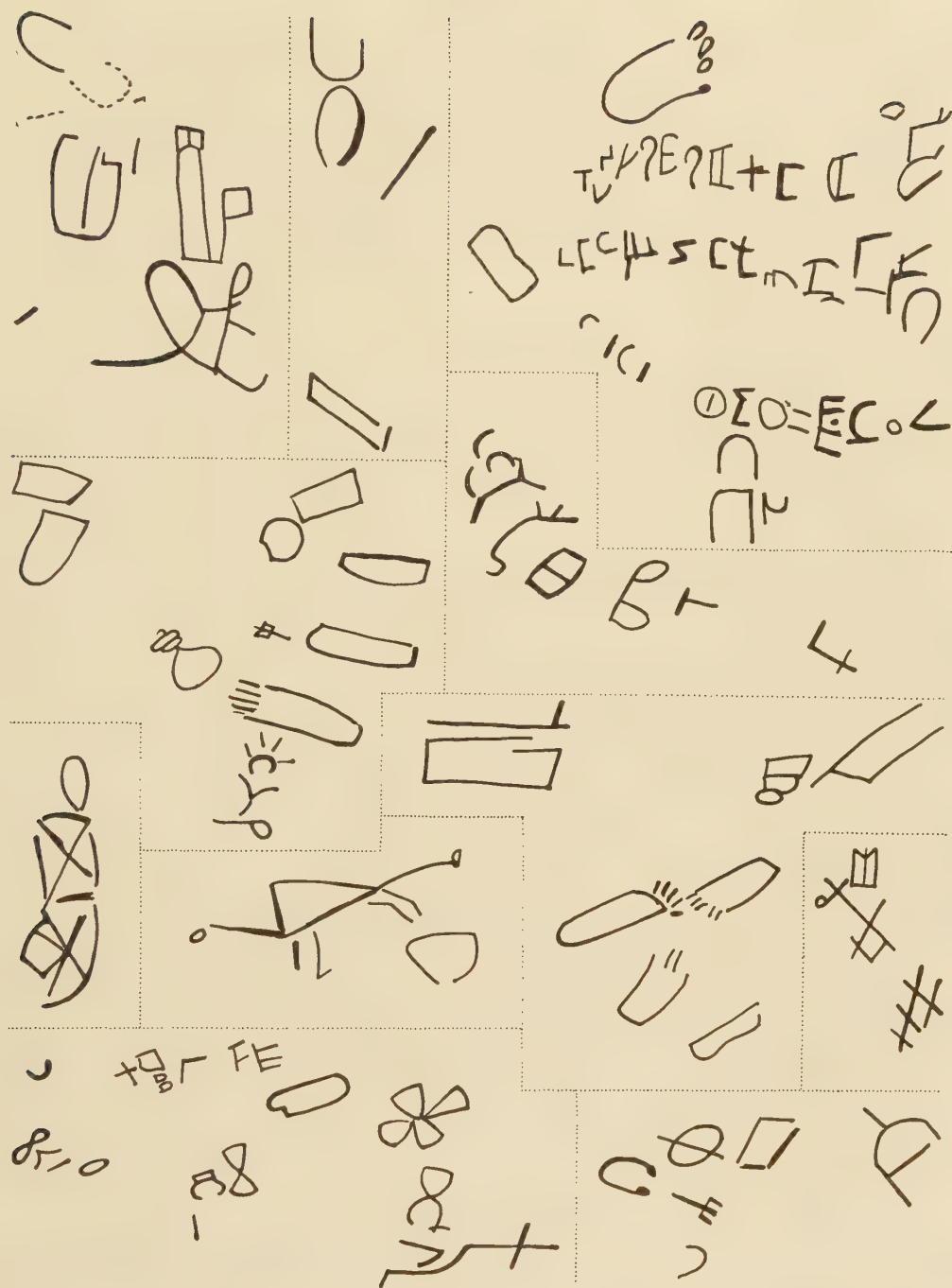


Fig. 7. The Eriosh inscriptions.

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APPENDIX

The antiquities found by the expedition consisting in the main of potsherds are now deposited in the Department of Archaeology of the University College of Ghana.

The only other pieces known to come from Socotra are:

1. A fragment of inscription said to be in Gujerati in the Aden Museum where it is numbered AM.350. This information was kindly supplied by Mr Charles Inge.
2. Wooden objects found by Bent in a burial cave. He describes them, somewhat inaccurately as 'having originally served as crosses' (*Southern Arabia*, p. 356). In fact they look much more like wooden clubs. They are in the Pitt-Rivers Museum in Oxford.

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THE MAKING OF THE BROADS. By J. M. LAMBERT, J. N. JENNINGS, C. T. SMITH, CHARLES GREEN and J. N. HUTCHINSON. pp. 153, 11 plates, 63 maps, plans, sections and diagrams. John Murray, 25s. Available from The Royal Geographical Society, Kensington Gore, London, S.W.7. 26s. 2d. postage included. A memoir in the Research series of the R.G.S. (No. 3): a reconsideration of the origin of the Broad's in the light of new evidence.

EXCAVATIONS AT JERICHO. Vol. I the Tombs Excavated in 1952-4. By Kathleen M. Kenyon. pp. 583, Frontispiece and 43 plates, 230 text-figures. British School of Archaeology in Jerusalem.

PERSIAN CITIES. By LAURENCE LOCKHART. pp. 188, 72 photographs (6 in colour), end-paper map. London, Luzac & Co. £3 3s.

YORK: A SURVEY 1959. Edited by G. F. WILMOTT, J. M. BIGGINS and P. M. TILLOTT. pp. 198, 16 maps and figures. Published by the Local Executive Committee on the occasion of the Meeting of the British Association in York, 1959. [Obtainable from the City Information Service, Public Library, Museum Street, York. 15s. post free.]

L'EXPANSION ET LA COLONISATION GRECQUES JUSQU'AUX GUERRES MÉDIQUES. By JEAN BERARD. pp. 178. Paris, Aubier (Collection Historique) 1960. 500 francs approximately.

BRITTANY. By P. R. GIOT. pp. 215, 68 text figures, 48 plates. London, Thames and Hudson, 1960. 30s. (Volume XIII in the *Ancient Peoples and Places* series.)

THE ANTECEDENTS OF MAN. By SIR W. E. LE GROS CLARK. pp. 374, 152 text figures. Edinburgh, the University Press, 1959. 21s. An Introduction to the Evolution of the Primates, based on the Munro Lectures delivered in Edinburgh in 1953.

DIE GLASARMRINGE UND RINGPERLEN DER MITTEL- UND SPÄTLATËNEZEIT AUF DEM EUROPÄISCHEN FESTLAND. By T. E. HAEVERNICK. pp. 302, 35 plates, figures and maps. Bonn, 1960, Rudolf Habelt for the Römisch Germanische Kommission. D.M. 65.

TELUL ETH THALATHAT, Vol. I. (The Tokyo University Iraq-Iran Expedition, Report I.) Excavation of Tell II, 1956-7. By PROFESSOR NAMIO EGAMI. pp. 195, 81 plates, 6 colour plates, 72 text figures, English summary of pp. 24. Tokio, Yamakawa Publishing Co., 1959. 12,000 yen.

EPITYMBION ROMAN HAKEN. Edited by J. FREL and others. pp. 142, 8 plates, 8 pages of line drawings. Prague, 1958. No price stated. Essays to the memory of Roman Haken (14 in German, 15 in English).

[continued on p. 137]

C-14 Dating and the Neolithic in Ireland

by W. A. WATTS

Dr Watts of the School of Botany, Trinity College, Dublin, here publishes for the first time the radio-carbon dates obtained in the C-14 laboratory of the University of Dublin, with special reference to the Neolithic in Ireland. He agrees with our Editorial comment (ANTIQUITY, 1959, 239-40 and 1960, 4-5) that we are still far from a position when a full new Neolithic chronology based on radio-carbon dating can be established, but his article, Professor Waterbolk's article published in our previous issue (ANTIQUITY, 1960, 14), and Dr Giot's note in the present issue (p. 147, infra) show that we are nearer than many of us thought.

FROM mid-1958 until February, 1960, a radio-carbon dating machine using scintillation techniques was in use in the Physics Department of Trinity College, Dublin. The machine was designed and built by C. F. Delaney, J. H. J. Poole and I. R. McAulay. Mr McAulay operated the machine, and the author assembled and prepared material for dating. Some seventy dates were obtained, of which those that relate to the Neolithic period are discussed in this article. Our object was to try to establish a few absolute dates for the Neolithic to help orientate chronological discussion. I assume that readers are already familiar with the difficulties and limitations of radio-carbon dating, both physical and archaeological,^{1,2,3} and the significance to be attributed to individual radio-carbon dates. In the text I suggest absolute dates for particular events. The suggestions, of course, are only as precise as the radio-carbon dates on which they are based permit. Naturally future work will make greater precision possible.

When did farming begin in Ireland, and what sort of relationships existed between the first farmers and the Mesolithic inhabitants? Three dates relate to this problem. At Dalkey Island, Co. Dublin, two shell-middens were covered by later archaeological material. Both contained a Larnian (Mesolithic), flint industry and 'limpet scoops'. This equipment, and the habit of eating shell-fish, distinguished the earliest inhabitants of the island from their immediate successors. Both middens contained polished stone axes, the southern yielding also bones of the usual domestic animals and a few sherds of Western Neolithic pottery. The north midden had no pottery, but had bones tentatively identified as sheep and dog. Its date was 3340 ± 170 B.C. (D 38). In layers overlying the two middens were flints of Neolithic type and pottery with ledge shoulders and horizontal out-turned rims, in some cases decorated by transverse scoring, that is related both to pottery of Lough Gur Class IA⁴ and to Lyles Hill ware.⁵ The evidence suggests that the Dalkey Island people were ethnically Larnian but that their culture had been overlaid by features borrowed from

¹ Barker, H., 'Radio Carbon Dating: Its Scope and Limitations', ANTIQUITY, 1958, 253-63.

² Tauber, H., *Acta Archaeologica Austriaca*, 1958, 24, 59-69.

³ Waterbolk, H. T., ANTIQUITY, 1960, 14-18.

⁴ Ó Ríordáin, S. P. 'Lough Gur Excavations: Neolithic and Bronze Age Houses on Knockadoon', *Proc. R. Irish Acad.*, 1954, 56, C 5.

⁵ Evans, E. E., 'Lyles Hill. A Late Neolithic Site in Co. Antrim'. H.M. Stationery Office, Belfast, 1953.

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Neolithic immigrants to whose way of life they were eventually assimilated. I am indebted to Dr G. D. Liversage for this information prior to publication. The site is of great importance in indicating the nature of the relationship between the earliest Neolithic inhabitants and at least some of the Larnians. Other recently excavated Irish sites show similar Mesolithic-Neolithic contact. At Sutton, Co. Dublin,⁶ a shell-midden on the raised beach contained Larnian flints and polished stone axes. Its period of use corresponded with the maximum of the marine transgression. Middens at Rockmarshall, Co. Louth,⁷ which were disturbed by the marine transgression at its highest point, contained Larnian flints only. The recently excavated site at Ringneill,⁸ Co. Down, produced bones of ox and pig, unaccompanied by bones of definitely wild animals, in a position indicating that the occupation site with which they were associated had later been covered by beach deposits of the transgressing sea. The remaining archaeological material consisted of flint which was small in quantity and not very distinctive. In Professor Jessen's opinion⁹ the maximum transgression of the sea in Ireland took place when the Atlantic period was giving way to the Sub-Boreal period. Three radio-carbon dated bog profiles suggest that the zone boundary should lie slightly before 3000 B.C. It is probable that the occupation of the four sites mentioned was roughly contemporary. At Sutton and Rockmarshall leaf-shaped flint points thinned at the bulbar end are found. They appear to be inferior versions of the Bann flake, well-known from the excavations of Movius¹⁰ at Newferry, Co. Antrim, where they occur with polished stone axes in hearths stratified in diatomite. Two hearths, one vertically above the other, are at present exposed on the east side of the River Bann, some 100 metres from Movius's excavation. Dr A. G. Smith, who is studying the palaeobotany of the site, drew my attention to the hearths. The upper hearth which contains Bann flakes and fragments of polished stone axes, gave a date of 3330 ± 170 B.C. (D 36). The lower contained flints of a similar, but not very distinctive, character and was separated from the upper by some 30 cm. of diatomite. It must be substantially older. Naturally many of the hearths at Newferry may also be younger, and the simultaneous use of Bann flakes and polished stone axes may have been of long duration.

In addition to Dalkey Island, Western Neolithic pottery is known in early contexts from other sites. At Mount Sandel, near Coleraine, Co. Derry, A. E. P. Collins has recently found some Western Neolithic sherds in apparent primary association with a splendid series of microliths, tranchet axes and other typically Mesolithic flint types. Our oldest date for a collection of Western Neolithic pottery is for Lyles Hill ware from Knockiveagh Round Cairn, Co. Down (3060 ± 170 B.C.—D 37). Trial excavations were made by A. E. P. Collins¹¹ at this site. The excavated portion of the cairn was found to cover an abundance of charcoal and broken sherds, all of Piggott's 'g' type carinated bowl. I think one is justified in making the following generalizations from the above evidence: A Neolithic economy was being practised in at least some parts of Ireland before 3000 B.C. At least some groups of Larnians adopted Neolithic techniques piecemeal and were ultimately absorbed into the Western Neolithic culture. To emphasize the fact that the Neolithic culture had already

⁶ Mitchell, G. F., 'An Early Kitchen-midden at Sutton, Co. Dublin', *J.R.S.A.I.*, 1956, **86**, 1-26.

⁷ Mitchell, G. F., 'An Early Kitchen-midden in Co. Louth'. *J. Co. Louth Archaeological Soc.*, 1947, **11**, 169-74.

⁸ Collins, A. E. P., and Stephens, N., *Proc. R. Irish Acad.* 1960, (in press.)

⁹ Jessen, K., 'Studies in Late Quaternary Deposits and Flora-history of Ireland', *Proc. R. Irish Acad.*, 1949, **52** B, 85-290.

¹⁰ Movius, H. L., *The Irish Stone Age*, Cambridge, 1942.

¹¹ Collins, A. E. P., 'Trial Excavations in a Round Cairn on Knockiveagh, Co. Down.' *U.J.A.*, 1957, **20**, 8-28.

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begun in the 4th millennium B.C., I suggest 3100 B.C. as an acceptable minimum date for the beginning of a full Neolithic culture in Ireland. On the evidence of Giot,¹² the Western Neolithic had already begun in Brittany by this date. If Windmill Hill is to represent the oldest Neolithic in Britain its date can hardly be less than 3000 B.C.

Lyles Hill ware and Lough Gur Class 1 and 1A are the major pottery types within the Western Neolithic tradition in Ireland. We obtained three dates from Lough Gur. Two came from 'Circle L',¹³ a house-site with two superimposed structures. The lower level contained Class 1 and 1A pottery and very probably some Class 2 ware, though the stratigraphy is very compressed and one cannot be absolutely certain that the Class 2 ware is not derived from the higher level, which is chiefly characterized by Beaker pottery. Dates were obtained from charcoal from the old ground level (2450 ± 240 B.C.—D 40), and from charcoal from postholes in the old ground level (2730 ± 240 B.C.—D 41). The Beaker level did not yield sufficient material for dating. The two dates may be combined, the large errors being due to the small quantities of charcoal available. A date of about 2600 B.C. seems likely for the earlier house. It is not necessarily the oldest house site at Lough Gur, but there is at least a suggestion that the Lough Gur occupation was later than the primary colonization in the north-east and along the east coast. The occupation layer at Geroid Island, Lough Gur¹⁴ yielded pottery which should be later than the period of use of Class 1A, precede Beaker, and be contemporary with Class 2 and the later period of use of Class 1 pottery. Charcoal from the site gave a date of 2130 ± 140 B.C. (D 39). As some Lough Gur sites yield food-vessel, the neighbourhood of the lake must have been continuously occupied for at least 1000 years. Nutbane Long Barrow was dated by the British Museum to 2721 ± 150 B.C.¹⁵ The early occupation of Lough Gur and the construction of Nutbane must have been more or less contemporary.

Western Neolithic pottery is usually associated with the 'court cairn'¹⁶ ('horned cairn') type of megalith. The forecourt of Ballyutoag cairn¹⁷ (Hanging Thorn Cairn), contained pottery of Lyles Hill ware, Beacharra ware and charcoal, which gave a date of 2160 ± 300 B.C. (D 48). The value of the date is limited by the large error, due to the small quantity of charcoal available, but the historical date probably lies in the second half of the 3rd millennium B.C. We have a few other indications of age for the court cairns. Aghanaglack,¹⁸ a double horned cairn in Co. Fermanagh, contained abundant charcoal of *Fraxinus* (ash), in primary position. The evidence of our radio-carbon-dated pollen-profiles from bogs shows that ash was a very rare tree, absent over most of the country, before 2500 B.C., when it began to appear in moderate quantity for the first time in the Central Plain. The court cairn of 'lobster-claw' form at Creevykeel, Co. Sligo¹⁹ contained clay balls which the excavator compared with the stone balls found in most passage graves of the Boyne culture. Passage graves were being built shortly before 2000 B.C., as I shall shortly show. This admittedly tenuous evidence suggests that at least some court cairns were in use during the second half of the 3rd millennium B.C. We have no data to indicate very early building of court

¹² Giot, P. R., 'Une station du Néolithique Primaire Armoricaire: Le Curnic en Guissény, Finistère,' *Bull. Soc. Préhist. Franc.*, 1959, **56**, 292-3.

¹³ Unpublished excavation of the late Professor Ó Ríordáin. To be published by Dr M. de Paor.

¹⁴ Liversage, G. D., 'An Island Site at Lough Gur', *J.R.S.A.I.*, 1958, **88**, 67-81.

¹⁵ *ANTIQUITY*, 1959, 289.

¹⁶ de Valera, R., 'The Court Cairns of Ireland', *Proc. R. Irish Acad.*, 1960, **60**, C 2.

¹⁷ Herring, I. J., 'The Forecourt, Hanging Thorn Cairn, Ballyutoag, Ligoniel', *Proc. Belfast Nat. Hist. and Phil. Soc.*, 1936-7 (1938), 43-9.

¹⁸ DAVIES, O., 'Excavation of a Horned Cairn at Aghanaglack, Co. Fermanagh', *J.R.S.A.I.*, 1939, **68**, 21-38.

¹⁹ Hencken, H. O'N., 'A Long Cairn at Creevykeel, Co. Sligo', *J.R.S.A.I.*, 1939, **69**, 53-98.

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cairns, though this is not excluded. One wonders if the usual association of Western Neolithic with other pottery classes in the cairns¹⁶ does not itself suggest that the cairns need not have been built in the earliest part of the Neolithic. Coarse-ware and cord-ornamented ware, both of which are well-represented in court cairns, have no claims to be regarded as early. A terminus is given to the cairns by the total absence of Beaker pottery or food-vessel and the apparent replacement of the court cairns by passage-graves in some areas. The passage-graves were in use by 2100 B.C. (see below). In the circumstances it is hard to imagine court cairns being built after 2100 B.C.

The replacement of Western Neolithic pottery by other styles is illustrated by several sites in Ireland. Western Neolithic pottery gave way to Sandhill wares at Goodland, Co Antrim,²⁰ a site recently described by Dr Case. Blanket-bog peat had sealed this ritual site where pottery had been buried in pits. The base of the peat gave a date of 2190 ± 200 B.C. (D 46). Taking the error of the date into account, the peat formation cannot have begun much later than 1900 B.C. and the ritual site must have been abandoned before, though not necessarily much before, that date. At Lough Gur, Western Neolithic pottery went out of use during Beaker times. The date is difficult to define, no datable material being available. If evidence from the Netherlands²¹ is relevant, the date should not be much later than 1800 B.C., by which time the Dutch beakers had more or less completed their development. At Lyles Hill⁵ Western Neolithic pottery, coarse-ware and cord-ornamented pottery were associated in a tomb which showed art of passage-grave type and some structural details reminiscent of passage-graves. On the evidence of passage-grave dates the site cannot be much older than 2100 B.C. and may be younger. To summarize, Western Neolithic pottery appears to have gone out of use in Ireland round about 1800 B.C.

In North-West Ireland Boyne Culture passage-graves seem to have succeeded court cairns as the main megalith type. The Carrowkeel cemetery shows a transition between the cultures in Cairn E.¹⁶ The evidence for dating the passage-graves has been confused by the frequency with which food-vessels have been found associated with them. The most recent excavations at Four Knocks²² and Tara (unpublished), make it clear that food-vessels in both instances were secondary. Hartnett²² has shown that the pestle-shaped stone pendants of the Boyne Culture are different in important respects from the amber beads of the Wessex Culture²³ with which they have been compared, and that there need be no cultural connection. Comparisons have been made between Boyne Culture art and decoration on early metal types. These decorative motifs could as easily be borrowed from Beaker pottery as from passage-grave art, and certainly do not provide clear evidence that the Boyne Culture people were metal-users. Our dating evidence for the culture is as follows: The Mound of the Hostages, Tara, Co. Meath, whose excavation was begun by the late Professor Séan Ó Ríordáin and completed by Professor de Valera, is a small passage-grave with the typical material equipment and art of the Boyne Culture. Under the structure was a ditch, preceding it in age. Charcoal from the ditch fill gave a date of 2120 ± 160 B.C. (D 42). The ground under the cairn had been burnt over prior to construction and the remnants of the burning were represented by a black smear over much of the old ground surface. This black material, consisting of finely comminuted charcoal and some carbonized seeds gave a date of 2300 ± 160 B.C. (D 43). Close to the entry to the passage a large fire had been lit

²⁰ Case, H., Paper to Prehistoric Society, 1960 (in press).

²¹ van der Waals, J. D. and Glasbergen, W., 'Beaker Types and their Distribution in the Netherlands', *Palaeohistoria*, 1955, 4, 5-46.

²² Hartnett, P. J., 'Excavation of a Passage-grave at Four-Knocks, Co. Meath', *Proc. R. Irish Acad.*, 1957, 58 C 5.

²³ Piggott, S., *Neolithic Cultures of the British Isles*, Cambridge, 1954.

on the old ground surface. According to Professor de Valera the fire should be contemporary with, or, at most, very slightly later than the building of the cairn. Charcoal from the fire gave a date of 1920 ± 150 B.C. (D 44). Considering the three dates it is practically certain that the passage-grave was built about 2100 B.C. At Moytirra, Co. Sligo,²⁴ a piece of Loughcrew (Boyne Culture) ware was associated with sherds of international bell-beakers in a cist-grave in a tumulus. Dutch evidence²¹ suggests a dating range of 2200–2000 B.C. for this type of beaker. As the beakers underwent a rapid stylistic development it is unlikely that the Dutch beakers antedate the Irish by very much. At Carrowkeel Mountain, Co. Sligo, a passage-grave cemetery was built on bare rock, now peat-covered. On the same mountain a Beaker wrist-bracer was found a few centimetres from the base of the peat.²⁵ The suggestion is that the passage-graves precede the Beaker find, though not necessarily by much. Pollen-diagrams from Treanscrabbagh bog, which lies in a valley on Carrowkeel Mountain, show a great increase in pollen of weeds of cultivation at about 2400 B.C. A large hearth of burnt stone and ash in the bog is of this age, but there are no finds. Some substantial agricultural activity seems to have begun at Carrowkeel about 2400 B.C., but, if we remember that the passage-grave cemetery contains one tomb of court-cairn type which should precede the passage-graves, we can suppose that the passage-graves were not built until some time after that date. Adjacent to the passage-grave at Four Knocks²² is Four Knocks 2, a ritual site which yielded Loughcrew ware and charcoal (1520 ± 140 B.C.—D 45). Taking the error of the date fully into account, the site is very unlikely to be older than 1800 B.C. and may be younger. On the basis of the evidence presented I suggest that the building of passage-graves had begun by about 2200 B.C. and was complete by 1600 B.C., if not earlier. From the archaeological viewpoint the culture should precede and be partly contemporary with beakers, and be succeeded by food-vessels.

We have few dates from the latest part of the Neolithic. The site with Sandhill pottery at Island McHugh, Co. Tyrone²⁶ gave a date of 1420 ± 120 B.C. (D 47). The historical date is therefore unlikely to be older than 1650 B.C. There would probably be fairly general agreement that this site should be very late in the Neolithic. Only one date was obtained for a megalith of 'wedge' type. The date was for Island, Co. Cork²⁷ (1150 ± 140 B.C.—D 49). The historical date for this material is very unlikely to be older than 1400 B.C. at the most. Wedges are strongly associated with Beaker pottery in Ireland, though Island produced no distinctive finds. The date seems much too young, but is just about conceivable. It is unfortunate that no comparative material was available. One Neolithic site produced a highly improbable result. A collection of Sandhill pottery from Dundrum sandhills,²⁸ Co. Down was dated by charcoal submitted by A. E. P. Collins (900 ± 140 B.C.—D 51). The date seems far too young to Dr Collins and to myself. The site contained Sandhill pottery and charcoal sealed by an old turf-line. A possible explanation is that the turf-line did not form until some time after the occupation of the site, and that at least some of the charcoal is a secondary intrusion.²⁹

I consider that three major conclusions of importance emerge from the above account: (1.) The Neolithic Period began before 3000 B.C. in Ireland, (2.) Lough Gur was occupied by 2600 B.C., (3.) At least some passage-graves had been built by 2100 B.C. These three state-

²⁴ de Paor, M., Discussion in paper to Hamburg Conference (in press).

²⁵ Unpublished, Wrist-bracer in National Museum, Dublin.

²⁶ Davies, O., *Excavations at Island McHugh*, Belfast (Nat. Hist. and Phil. Soc.), 1950.

²⁷ O'Kelly, M. J., 'A Wedge-shaped Gallery-grave at Island, Co. Cork', *J.R.S.A.I.*, 1958, **88**, 1–24.

²⁸ Unpublished.

²⁹ Dates in this paper from Ireland are based on the oxalic acid standard, except D 46, which is based on a closely comparable standard. In comparing Dutch and Irish dates I have added 200 years to the Dutch. (*See n. 3.*)

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ments are the most important results of our work. We agree with Dr Daniel's view that we are still far from a position when a full Neolithic chronology based on radio-carbon dating can be established, but can we not take a first step in that direction by accepting the evidence now available from several countries, that the Neolithic began very much earlier than has commonly been supposed?

In his March editorial Dr Daniel wonders whether Carbon-14 laboratories may not find themselves unemployed in the near future with all problems solved, and goes on to give his opinion that such a view is far too optimistic. I think these observations may arise in part from conversation with myself and I should like to clarify my attitude. Carbon-14 machines are more likely to stop work for lack of first-class samples to date than for lack of problems. Useful dates can only be obtained from sites which yield sufficient carbon in absolutely unambiguous stratigraphical position and which illuminate a clearly defined chronological problem. In my experience very little first-class dating material is produced annually by excavation and material from old excavations is hardly ever well enough documented to be usable. For example, the few dates we have been able to produce from the Irish Neolithic have exhausted practically all the datable material for that period. In these circumstances there is a temptation to date material which is not really well enough documented. If one does, inevitably incongruous and unacceptable dates are produced which can be used to make criticism of radio-carbon dating as a whole intellectually respectable. I suggest that the future of radio-carbon dating will lie with large teams. One envisages groups of archaeologists, with well-defined chronological problems to solve, conducting digs to provide their scientific colleagues with first-class dating material. A Carbon-14 group should consist of both archaeologists and physicists working in close contact. At present the contact between physicist and archaeologist may only be by post and misunderstanding may easily arise. While it is excellent that archaeologists should be able to send material to laboratories for dating, there is a danger that haphazard production of dates from miscellaneous material will fail to yield any sort of chronological picture. As Waterbolk says 'a single date is no date'. A systematic approach, only possible with a reasonably large number of collaborators, is what is needed to solve all but the easiest problems. In the meantime there is a place for small teams like ourselves to work on limited problems with the available material and provide a few chronological indications of value.

I should like to thank all those Irish archaeologists who kindly helped us with material and discussion. I should also like to acknowledge that we were financed by the Wenner-Gren Foundation, the Royal Irish Academy, the Royal Dublin Society and Trinity College, Dublin, and to express our thanks to these bodies.

Egypt's Earliest Sailing Ships*

by RICHARD LeBARON BOWEN, Jr.

Dr Bowen is a chemical engineer who, while working in Saudi Arabia for the Arabian American Oil Company in 1945-7 became interested in primitive and native watercraft. In 1950 he joined the American Foundation for the Study of Man archaeological expedition to South Arabia, where he made a study of ancient irrigation. Together with F. Albright he wrote Archaeological Discoveries in South Arabia (Baltimore, 1958).

ALL interpretative works on the prehistory of the ancient Near East tell us that the earliest representation of a sailing ship is that painted on an Egyptian vase of the predynastic period in the British Museum (FIG. 1).¹ This is not true, for there is an

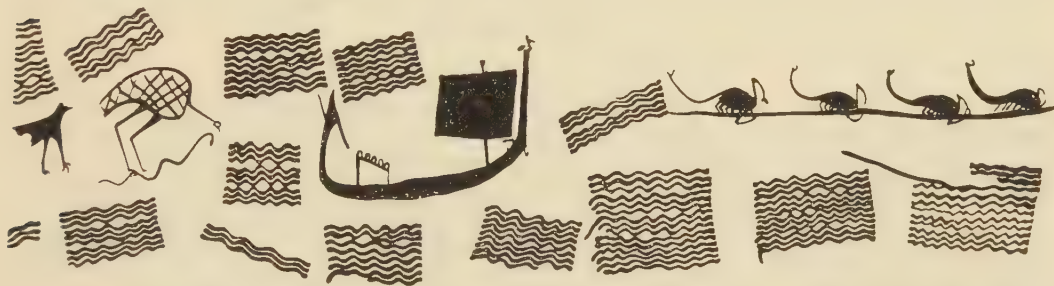


Fig. 1. Design painted on Egyptian decorated vase (red painting on buff ware). This sail-boat has previously been called the world's oldest representation of a sail. The vase can be placed at the very end of the Gerzean period, just before the start of the First Dynasty, about 3100 B.C. The mast is located 19 per cent of the overall length from the bow. [British Museum, No. 35324, 23 in. high.] (After Frankfort, *Studies in Early Pottery of the Near East*.)

earlier picture of a sail-boat to be found on another painted Egyptian vase which has remained unrecognized. Ironically, this vase is exhibited in the British Museum right beside the better known example (FIG. 2).² Although both of these vases are of unknown

* The author wishes to express his appreciation to Professor Richard A. Parker of Brown University (U.S.A.) and to Professor George F. Carter of Johns Hopkins University (U.S.A.) for discussing many of the problems considered in this study.

¹ E. A. W. Budge, *A History of Egypt* (London, 1902), I, 80; H. R. Hall, *The Civilization of Greece in the Bronze Age* (London, 1928), 36, fig. 28; V. G. Childe, *What Happened in History* (New York, 1942), 75; S. Lloyd and F. Safar, 'Eridu', *Sumer*, IV (1948), 115-27; R. D. Barnett, 'Early Shipping in the Near East', *ANTIQUITY* (1958), 222. British Museum vase No. 35324, purchased in 1901, provenance unknown. Drawing on pot: H. Frankfort, *Studies in Early Pottery of the Near East* (London, 1924), I, pl. XIII (1).

² British Museum vase No. 36326, purchased in 1902, provenance unknown. The author wishes to thank Dr I. E. S. Edwards, Keeper of the Department of Egyptian Antiquities, British Museum, for allowing him to examine this vase in November, 1957; and Mr A. F. Shore, for arranging for photographs. E. A. W. Budge, *op cit.*, 73, published a line drawing of the boat on this vase without describing it, and failed to recognize the sail.



Fig. 2. Egyptian decorated vase (red painting on buff ware) of the Gerzean period showing a sail-boat with tall rectangular sail on a pole mast, located about 16 per cent of the overall length from the bow. The vase can be placed squarely in the Late Gerzean period, probably a hundred years before the start of the First Dynasty, or 3200 B.C. in very round numbers. [British Museum, No. 36326, 18 in. high.] (Author's drawings.)

provenance, they fortunately have shapes and styles which enable them to be placed chronologically with a fair degree of accuracy. Both are done in a technique known as 'Decorated ware', a term invented by Sir Flinders Petrie for the red-painted buff ware. The one previously recognized as the earliest representation of a sail has a shape which belongs just before the 1st Dynasty, at the very end of the Gerzean, the last of the Egyptian predynastic periods.³ The style of the painting of this vase also differs from the earlier typical Gerzean designs in certain details, such as the cross-hatched bird. The other vase has no features which definitely separate it from the very typical Decorated ware of the Gerzean period. Its shape, the flamingoes, the gazelles, the triangles and wavy lines are all found on typical Gerzean ware. However, the shape of the vase, the triangular lug handles, and the painted triangles are all limited to the last half of the Gerzean, the Late Gerzean (S.D. 46-63). The boat is also very typically done, with its many oars looking like a centipede.⁴ If we place the start of the 1st Dynasty in Egypt at 3100 B.C., then we can date our first vase to this time. It is only a guess that the Late Gerzean period may have covered a span of a couple of hundred years. If this were true our second vase could be easily a hundred years earlier, say 3200 B.C. in very round numbers.

There is another Decorated vase which should also be considered (FIG. 3). This was excavated at Naqada in Upper Egypt,⁵ and is now in the Ashmolean Museum, Oxford.⁶

³ H. J. Kantor, 'The Final Phase of Predynastic Culture', *Journal of Near Eastern Studies*, III (1944), 116. R. D. Barnett, *op. cit.*, 222, is mistaken in assigning this vase to the Amratian period.

⁴ It is hoped that no reader will open up the question again as to whether this is a fort or palisade or ostrich farm or some other fanciful interpretation instead of a boat. All those who have considered the problem in any detail in the last quarter-century consider them to be boats. Admittedly they are crude stylized representations. For a recent discussion of this problem see A. J. Arkell, 'Early Shipping in Egypt', *ANTIQUITY* (1959), 52-53.

⁵ W. M. F. Petrie and J. E. Quibell, *Naqada and Ballas* (London, 1896), pl. LXVII, 14, which shows the design only on the vase. E. J. Baumgartel, *The Cultures of Prehistoric Egypt* (2nd ed., London, 1955), pl. XI, has published a photograph of the vase.

⁶ Ashmolean Museum, No. 1895.584.

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Fig. 3. Two many-oared boats each with three steering oars painted on a decorated vase of the Late Gerzean period. The enigmatical design over the front of the left boat has been described as an animal skin, or an animal in some strange position. It is identical to other designs on these vases which are described as shields. [Ashmolean Museum, Oxford, No. 1895.584, 10 in. high.] (After Petrie and Quibell, Naqada and Ballas.)

Here we see the typical many-oared Gerzean boat design. Over the front of one of the two boats on the vase we see a strange object. Baumgartel, who has been the only one to my knowledge to discuss this detail, says that it could be the skin of an animal with head, tail, and four legs stretched out.⁷ But she says that it could also be a live animal in some unusual aspect. The shape of this vase is the same as that of the unpublished British Museum vase, so that it can be placed with assurance in the Late Gerzean.

Another design found on these Decorated vases may have a bearing on this matter. There are dozens of examples of what has generally been described as a 'shield' found painted on these vases (FIG. 4).⁸ These are characteristically placed below the boats between designs similar to the two structures on the boats. Petrie suggested at first that the objects looked almost like masts and sails.⁹ However, many scholars now agree that these objects are probably shields made of skins, which by analogy with similar weapons of the modern Dinkas, Baris, and Kaffirs of Africa would be secured by means of a long pole for ceremonial purposes.¹⁰ There is evidence that the two rectangular structures placed in the middle of the boats had a religious significance of some sort, and thus are more properly called shrines than cabins.¹¹ The fact that the shields are usually placed between two of these shrines would seem to indicate that they also were venerated objects.¹² Further, the fact that the shields are invariably found on pots with boats might seem to indicate that they played some part in ceremonies connected with these boats.

Do we not have evidence in these shields for the accidental invention of the sail? Is not the picture of this event at the very dawn of civilization in Egypt unfolding right before our eyes in these pottery drawings? If a shield made of the skin of an animal were at times set up on a boat during religious ceremonies like a banner, it would only be a matter of time

⁷ E. J. Baumgartel, *op. cit.*, 82.

⁸ W. M. F. Petrie, *Prehistoric Egypt* (London, 1920), pl. XIX, 41N, 41S, 41U.

⁹ W. M. F. Petrie and J. E. Quibell, *Naqada and Ballas* (London, 1896), 49.

¹⁰ J. Capart, *Primitive Art in Egypt* (London, 1905), 121.

¹¹ E. J. Baumgartel, *op. cit.*, 13.

¹² *Ibid.*

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until man observed that the shield would motivate the boat. Obviously any explanation showing that the invention of a device such as the sail could have been an accidental discovery is very desirable. The evidence possibly points in this direction. The learned consensus takes the object between the two shrines to be a shield. Petrie specifically took issue with this view, saying that no shield would ever have a pole projecting below it, or be slung from a pole; he defended his earlier suggestion that it was a sail.¹³ He pointed out that the great difficulty with such a suggestion is the fact that it was never shown on a boat. But is this observation correct? We have shown that an enigmatic object is found over the

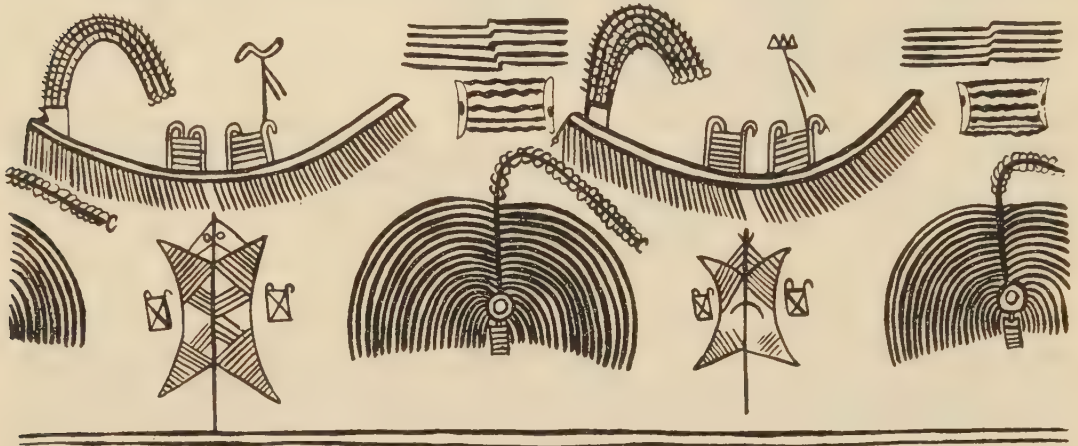


Fig. 4. Designs painted on a decorated vase of the Gerzean period showing two boats as the main motif. Below the boats, and between two structures similar to those found on the boats, are objects described as shields. The flowering plants have been identified as aloe plants in pots. [University College, London.] (After Petrie, Prehistoric Egypt.)

front of a Gerzean boat (FIG. 3). It has been suggested that it could be either the skin of an animal, or a live animal in some strange position. But there are no other representations of animals in such a rendering. On the other hand, the object over this boat is identical in shape to those objects described as shields, which may be animal skins. Therefore we could conclude that this object is a shield represented in the general area of the front of the boat. There is a flamingo in front of the base of the vertical pole ('tail' of animal skin), which prevents us from seeing whether or not this shield was erected on the boat.

There does not seem to be much question about the sail of the unpublished British Museum example (FIG. 2). It is a tall rectangular sail set on a pole mast which clearly goes down to the deck. One could object that this sail has relatively straight edges, and thus has no connection with the shields, which usually show the 'legs' of the animal as pronounced projections. However, some of the shields painted on the Decorated vases are actually in this same rectangular form (FIG. 5).¹⁴ Therefore the earliest definite sail can be equated with the shields found below some of the boats. If such is actually the case, then the sail may well have been invented accidentally in the Nile valley from the custom of placing in these

¹³ W. M. F. Petrie, *Prehistoric Egypt* (London, 1920), 21.

¹⁴ *Ibid.*, pl. XIX, 41C.

PLATE IX



THE MULTIPLE BRUSH

(a) *Predynastic Egyptian 'Decorated.' Triple brush. (London, B.M. 35324.)* (b) *Cypriot Middle Bronze Age, Red-on-Black. Brush with 12 members. (Oxford, 1953, 220.)* (c) *Cypriot Late Bronze Age, Base-Ring II. Quintuple brush. (Oxford, C.91, from Lakshà tu Riù.)* (d) *Cypriot Late Bronze Age, White Slip I. Quadruple brush. (Oxford, 1911, 331.)*

PLATE X



(a) *Attic Geometric, later 8th century. Quintuple brush. (British School at Athens.)*



(b) *Protocorinthian, earlier 7th century. Brush with 17 members. (Oxford, 1927, 24, from Cumae.)*



(c) *Boeotian or Euboean, 7th century. Sextuple, double and treble brushes. (Athens, N.M. 135.)*



(d) *Cyprus, from Marium. Chian chalice, first half of the 6th century. Quadruple brush. (Block lent by the Hellenic Society.)*

THE MULTIPLE BRUSH



THE MULTIPLE BRUSH

*Early Proto-Attic, about 700 B.C. Brush with 12 members. (Athens, Vlasto Collection.)
(Block lent by the British School at Athens.)*

PLATE XII



SOCOTRA

(a) Suk mosque. (b) South-East bastion of Suk fort.



SOCOTRA

Old houses in the hills.

PLATE XIV



SOCOTRA

Feragey fort. The top picture shows part of the main front wall of large boulders.



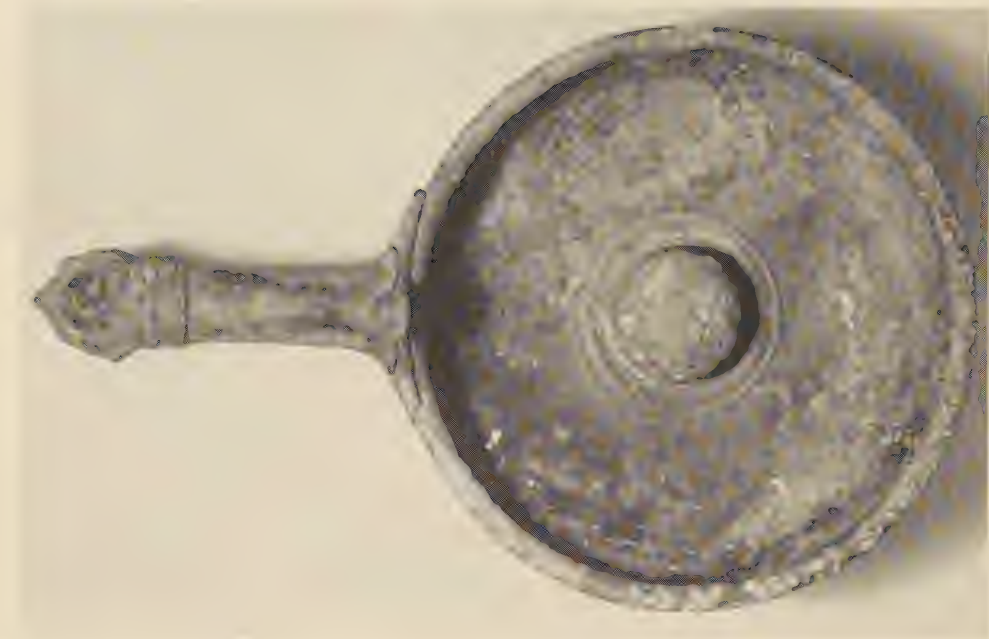
WELSHPOOL

An ox-head escutcheon from a bronze-mounted wooden bucket: total length 3.75 in.



(a) *Bronze ewer, 9 in. high.*

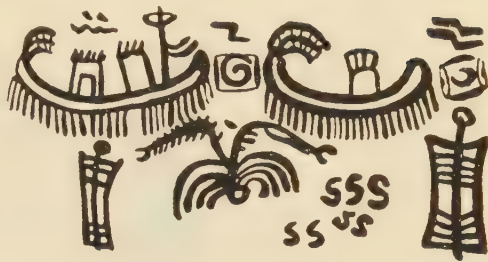
WELSHPOOL



(b) *Bronze patra or skillet, 7 in. in diameter, partly inlaid with silver.*

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Fig. 5. Designs painted on a decorated vase of the Gerzean period showing two boats as the main motif. The two shields below the boats differ from most of the other shields in being almost rectangular without any pronounced curve at the sides. The vase is in the University College, London. (After Petrie, *Prehistoric Egypt*.)



boats ceremonial shields hung from poles like banners. If man's first sail was a shield hung from a pole, it is evident that the discovery must have taken place on a relatively small boat. If we assume that a shield was 4 ft. or 5 ft. long, it would have to be on a small boat to have any driving force.

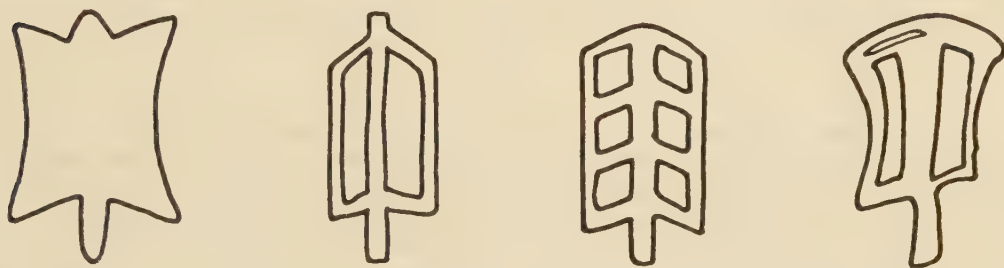


Fig. 6. Egyptian hieroglyphs from the First Dynasty used in personal names. The context indicates that these represent animal skins with the phonetic value *S'B*. They also bear a remarkable resemblance to the predynastic 'shield'. (After Petrie, *Royal Tombs of the First Dynasty*.)

There are a number of hieroglyphs found in personal names in seals of the 1st Dynasty which bear a remarkable resemblance to the predynastic shield (FIG. 6).¹⁵ However, in view of the context Egyptologists are in general agreement that this hieroglyph is an animal skin, with the phonetic value *S'B*. Professor Richard A. Parker of Brown University has suggested to me that perhaps the shield was originally called *S'B*. The Hierakonpolis wall painting (Late Gerzean) actually shows that before the 1st Dynasty shields were shown as animal skins with tails, for here we see a man defending himself with a shield made of a leopard skin (FIG. 7).¹⁶ The general shape of this shield is so similar to the predynastic shields shown on the Decorated pottery and to the animal skin hieroglyph of the 1st Dynasty that we can reasonably equate the three. The 'pole' which runs up the centre of the archaic 1st Dynasty hieroglyph *S'B* seems to indicate that they were indeed shields (skins) hung on a pole. If the bottom projection is to be dismissed as a tail, then its extension through the middle of the rectangle is difficult to explain. We do not find hieroglyphs of

¹⁵ W. M. F. Petrie, *Royal Tombs of the First Dynasty* (London, 1900), I, pls. XXVII, 64, 65, 66, 67; XXXVI, 48.

¹⁶ J. E. Quibell and F. W. Green, *Hierakonpolis* (London, 1902), II, pls. LXXV, LXXVI.



Fig. 7. Part of the famous wall painting from the Late Gerzean found at Hierakonpolis in Upper Egypt. Here a man defends himself with a shield made of a leopard skin, while the other man wears a shirt of a similar skin. This shield is almost identical to the 'shields' painted on the Decorated pottery. (After Quibell and Green, Hierakonpolis.)

sails until much later, in the Old Kingdom.¹⁷ However, neither the archaic hieroglyphs of the animal skin nor the first hieroglyphs of the sail help us much with the origin of the sail.



There is certain typological evidence preserved in early Egyptian pictures of sails which may indicate that the first sail indeed had a shield as an ancestor. The most convincing is the fact that the Egyptian sail was confined between a yard *and* a boom from the earliest examples showing us the details up to about 1200 B.C. At this time Egyptian sails appear for the first time without booms in the well-known relief of the naval battle of Rameses III (FIG. 8).¹⁸ A boom does not offer any particular advantage in a primitive rig, and in fact it is cumbersome in adding additional gear and is a hazard to the personnel. But it was apparently maintained for over 2000 years on the Nile because it was the traditional Egyptian way of setting a sail, and sailors and boat builders are among the most conservative and resistant to change of any group.

Grazer, who was one of the first to write about Egyptian ships, thought that the boom did not occur until the 12th Dynasty.¹⁹ But Assmann has shown that the boom was common in the Old Kingdom.²⁰ It has recently been suggested that the boom did not occur until the 6th Dynasty, all Egyptian sails before that supposedly being loose-footed.²¹ The evidence for such a statement is a fragment of a re-used Old Kingdom relief in the Metropolitan Museum, New York, showing the bows of three ships and a single great sail. The published photograph of this relief does not show the foot of the sail clearly.²² A large photograph

¹⁷ E. Assmann, 'Die Schiffsbilder', in L. Borchardt, *Das Grabdenkmal des Königs Sahu-re* (Leipzig, 1913), 161, points out that the earliest known hieroglyph of a sail is shown in a 3rd Dynasty relief published by W. M. F. Petrie, *Medum* (London, 1892), pl. xiv. No earlier one has been turned up since Assmann wrote. By the 5th Dynasty the mast is shown at times as a sheer mast in the hieroglyph (*ibid.*, fig. 21).

¹⁸ H. H. Nelson, 'The Naval Battle Pictured at Medinet Habu', *Journal of Near Eastern Studies*, II (1943); 40-55.

¹⁹ B. Graser, 'Das Seewesen der alten Ägypter', in J. Dümichen, *Resultate der . . . im Sommer 1868*, etc. (Berlin, 1869), I, 1-26.

²⁰ E. Assmann, *op cit.*, 157.

²¹ R. O. Faulkner, 'Egyptian Seagoing Ships', *Journal of Egyptian Archaeology*, xxvi (1940), 6.

²² *Bulletin of the Metropolitan Museum of Art*, December 1922, Part II, 13, fig. 12. Faulkner mistakenly gave 1921, 26, fig. 9 as the reference.

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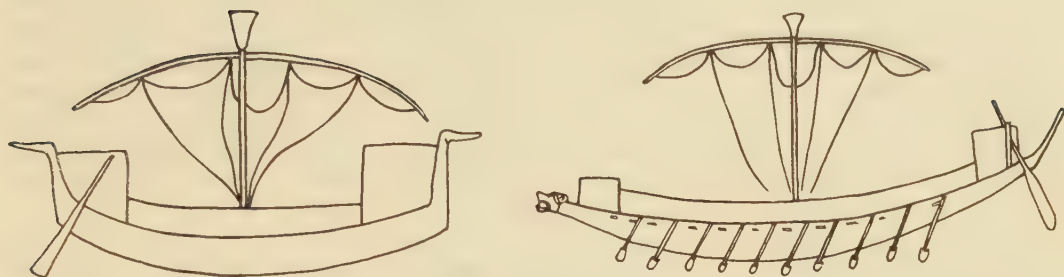


Fig. 8. Ships from a Twentieth Dynasty relief showing the first recorded naval battle, about 1200 B.C. in round numbers. The typical Egyptian ship is shown at the right, the 'Northern' enemy ships at the left. Both sail rigs are identical, with yards fixed above, sails furled, and without any booms. (Modified from Nelson, *Journal of Near Eastern Studies*.)

of the relief shows the sail passing down behind the crew, mast, and paddles, without showing either the foot of the sail or a boom.²³ This was a convention used in representing many Old Kingdom sails, and does not indicate that the sail was necessarily loose-footed. Proof that the boom existed before the 6th Dynasty is shown by the 4th Dynasty reliefs of sail boats with booms published by Assmann.²⁴ These are the earliest representations of sails showing enough details to tell whether or not a boom is present. It seems unreasonable to suggest that the Egyptians first used sails without a boom, then adopted one in the Old Kingdom, only to abandon it again in the New Kingdom. Therefore we are justified in assuming that the first Egyptian sails had booms.

No one has ever hazarded a guess as to why the Egyptians used a boom at the foot of their sails. It could be vestigial evidence showing that the first sails were copied from shields. Obviously a shield made of the skin of an animal had to have some sort of frame to stretch it out. It is evident from many of the shields shown on the predynastic Decorated pottery that the frame must have consisted basically of four light poles tied together at their ends. The poles would bend in from the ends and tend to keep the skin taut as well as utilize the area of the legs. When the shield was converted to a sail, the side poles were soon abandoned and the top and bottom ones remained as the yard and boom.

There is another characteristic of Egyptian sails of the Old Kingdom which may be further vestigial evidence of a shield. Many of the Old Kingdom yards curve up in a gentle sweep. Probably the best example of this crescentic yard is shown by a number of sailing ships from a 6th Dynasty relief at Deir el-Gebrawi (FIG. 9).²⁵ Proof that these curved yards were not some sort of an illusion is shown by the fact that models of such yards have been preserved.²⁶ It is difficult to see how such a shape could have any useful functional purpose. Actually such a curved yard requires additional gear to hold the ends up, for these would naturally tend to roll down if the yard were lifted only by a halyard. But a curved yard can logically be explained by assuming that it preserves the upper curved pole stretched to that shape by the skin. If man were to sit down and invent a yard for lifting a square sail, he

²³ Metropolitan Museum of Art photograph No. 63650, Ac. no. 21.1.13.

²⁴ E. Assmann, *op cit.*, fig. 20, 159. Actually the royal barge of 5th Dynasty king Sahure is clearly shown with a boom at the foot (*ibid.*, pl. 1X). This boat has been magnificently reconstructed in a coloured drawing in *LIFE* magazine for 1 October, 1956.

²⁵ N. de G. Davies, *The Rock Tombs of Deir el-Gebrawi* (London, 1902), vol. II, pl. VII.

²⁶ G. A. Reisner, *Models of Ships and Boats* (Cairo, 1913), 59-60, pl. XXVIII, 4891, 4892.

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would very probably arrive at a straight member raised by a single halyard. In most places in the world, in prehistoric as well as historic times, yards were straight. The edges of some 6th Dynasty sails also curve in (FIG. 9), possibly preserving the curved sides of the predynastic shields, although this could represent the wind billowing out the sail. The obvious shape of the sides of a square sail is straight. These curved yards are not the rule, and many other Old Kingdom sails have straight yards and the sails are perfectly rectangular. The curves in the tops of certain sails may preserve the older custom.



Fig. 9. Sixth Dynasty sailing ships from a relief at Deir el-Gebrawi in Middle Egypt. Note that the yards curve up in a crescentic shape, and have lifts. The edges of the sails also have a similar shape. (After Davies, *The Rock Tombs of Deir el-Gebrawi*.)

The lifts found on many yards of the Old Kingdom can also be explained by returning to the shield shown on the predynastic Decorated ware. Here the shield seems to be suspended from the vertical pole by a number of lines leading from the peak of the pole to points along the top of the shield (FIG. 4). These lines constitute lifts in nautical parlance. Such lifts would be essential for holding up a curved yard (FIG. 9). It is interesting to note that in those Old Kingdom ships with straight yards there are no lifts. So the curved yards with lifts may represent the older tradition, which went out of fashion after the Old Kingdom.

It has been suggested that the sail was present in Mesopotamia and Palestine before it occurred in Egypt. These statements were based on the previously published British Museum vase (FIG. 1), but the unpublished British Museum vase would not alter these assertions (FIG. 2), because the Mesopotamian and Palestinian evidence is still earlier. The first of this evidence is furnished by a pottery boat model from the last of the Ubaid or the first of the Uruk period at Eridu in southern Mesopotamia.²⁷ This would be earlier than the earliest representation of a sail in Egypt. There is a round socket in the centre of the model and it has been suggested that this held a mast and sail. However, the socket could also hold a religious standard or even a figurine, both of which are shown in representations in early Mesopotamia; but a sail is not represented until Assyrian times.²⁸ The Eridu model

²⁷ S. Lloyd and F. Safar, 'Eridu', *Sumer*, IV (1948), 115-7, pl. v. R. D. Barnett, *op. cit.*, p. 221, is mistaken in saying that the model was found with mast and stays.

²⁸ A. Salonen, 'Die Wasserfahrzeuge in Babylonien', *Studia Orientalia*, VIII, 4 (1939), p. 110, says that a cylinder seal of the Jemdet Nasr period (pl. III, 4) shows a mast and yard. However, a comparison of this with an earlier seal from the Uruk period (pl. III, 3) shows that the two lines ('mast' and 'yard') are a stylization of a punting pole and a steering oar.

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has a hole at each side of the gunwale, and it was suggested that these were for shrouds. But no Egyptian ship is ever shown with shrouds, and most primitive craft do not bother with them. The socket is located 38 per cent of the overall length of the boat from the bow. We will show below that such an arrangement did not occur in Egypt until well after 2500 B.C., perhaps a thousand years after the Eridu model. Earlier Egyptian craft had the mast much farther forward. Therefore, it is far from proven that this model from Eridu represents a sail boat.

It has been stated that the sail was illustrated in Palestine during the Ghassulian period, which would be earlier than the first Egyptian sails. Petrie said that there was a small two-masted boat between the arms of a great star painted in a fresco, and he said that there were two other possible boats.²⁹ If one turns the design upside-down there is possibly a two-masted boat, but not one single element of the details which can be understood in the rest of the painting is upside down. Further, the idea of two masts was not known in the Mediterranean until 3000 years later, and the sails would have to be lateen sails which were not known until 4000 years later. The excavators said that this object was a musical instrument.³⁰ Whatever it is, it is not a sailboat.



The question of whether or not the sail was actually invented accidentally in the Nile valley is certainly open to debate. There are many scholars who may feel that Egyptian evidence is very misleading when used as proof of man's first use of any particular phase of material culture. This is particularly true of the case in hand. Our first proof of the existence of sails anywhere in the world comes from the Bronze Age of Egypt. But does this mean that the sail was invented in this area? Some scholars will be quick to suggest that this is perhaps an illusion created by accidents of preservation and the intensity of investigation in the area. We simply could know too much about Egypt, and this could warp our thinking. Why could not Neolithic or even Paleolithic man have invented the sail? We have presented certain evidence above to show that the sail may have been invented in the Nile valley. But even if we assume that it was invented elsewhere, the sail rig we first see on the Nile was very primitive and took hundreds of years to develop to a practical means of transportation, as we shall see.

Egypt has always constituted the chronological framework for the ancient Near East. Both in prehistoric and historic times, the clear-cut sequence of Egyptian periods provides a basis for establishing the chronology, both relative and absolute, of certain other parts of the Near East.³¹ Egyptian chronology has been fixed astronomically back to about 2000 B.C., and can be extended as far as 3000 B.C. with a fair degree of accuracy. Just as Egypt provides a gauge for general chronologies, so also Egypt provides an accurate indication of the development of the sail in the general area of the Eastern Mediterranean. We are going to assume in the following discussion that all of the major improvements in sails taking place in the Eastern Mediterranean are shown in the Egyptian record. This is a valid assumption, since Egypt was in continual contact with all of the countries of the Eastern Mediterranean. Further, I have shown that sail forms are one of the most easily spread characteristics of man's material culture.³² We have no way of telling if the innovations

²⁹ W. M. F. Petrie, review of A. Mallon, *et al*, *Teleilat Ghassul*, in *Ancient Egypt*, 1934, 118.

³⁰ A. Mallon, R. Koeppl and R. Neuville, *Teleilat Ghassul* (Rome, 1934), frontispiece, 139.

³¹ H. J. Kantor, 'The Chronology of Egypt', in R. W. Ehrich, *Relative Chronologies in Old World Archaeology* (Chicago, 1954), 2.

³² R. L. Bowen, Jr., 'Eastern Sail Affinities', *American Neptune*, XIII (1953), 82; R. L. Bowen, Jr., 'The Origins of Fore-and-Aft Rigs', *American Neptune*, xix (1959), 294.

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appearing in the Egyptian record were made on the Nile or not, but some obviously were not.

There may have been a lag of dozens or even hundreds of years before certain improvements are found in the Egyptian record (and some are obviously not found at all), but this does not bother us seriously. We have definite proof from later times that the spread was actually very fast. In one instance we see some ships identified as Syrian vessels of about 1300 B.C.³³ The sails of these ships are identical to those of Hatshepsut's ships of a few hundred years earlier. Then we have the ships of the famous naval battle of Rameses III of about 1200 B.C. Here the rigs of both Egyptian and Northern ships are identical and unlike anything ever seen in Egypt before (FIG. 8). This rig may well have been due to the Mycenaeans, or some other Northerners. If we are allowed to extend this comparison back in time, we must suppose that the Egyptian pictorial record shows in general the approximate state of the development of the sail in the Eastern Mediterranean. The characteristic features of ancient sails which we now call 'Egyptian' are only Egyptian because we do not have any record of what other people associated with the Egyptians used in the earliest times. But if our surmise is correct, they were closely related to contemporary Egyptian customs, so far as the general stage of development of the sail went.

Whether this line of reasoning can be applied to Mesopotamia, India, and the eastern part of the Indian Ocean we have no way of telling for sure. But there is modern evidence which indicates that it was so, for all over the Indian Ocean and the western Pacific we find sail forms and rigs which immediately remind us of the ancient Egyptian record. These have possibly been preserved from ancient times. We find tall rectangular sails like those used in the Old Kingdom, and we find wide sails like those used in the New Kingdom. We find a tall rectangular sail in northern China, Japan, Korea, and certain of the Melanesian islands. This could represent the limits of the earliest spread of the typical sail of the Old Kingdom (c. 2660-2180 B.C.). We find the sheer mast, the tripod mast, and the related mast shore (prop) in Indonesia, south-eastern Siberia, Micronesia, and Polynesia. The sheer and tripod masts are also limited to the Old Kingdom in Egypt. Then we find the wide rectangular sail typical of the New Kingdom (c. 1570-1075 B.C.) from India to Indonesia, possibly indicating a later spread which replaced the tall sail. There is little doubt that the first appearance of these characteristics is indicated roughly by the Egyptian record. But these features are 'Egyptian' only because their much more widespread use in *ancient* times is preserved in Egypt.

The fact that such forms are preserved in Egypt should not lead us to assert that they all spread out of Egypt. Perhaps some of the new developments were even perfected in the Indian Ocean, and passed from the Indus civilization to Egypt, perhaps through Mesopotamia, or even directly. But the fact that they exist today shows quite clearly that their initial spread certainly must be indicated by the Egyptian framework, if only roughly. If the new characteristics were borrowed by the Egyptians, it may be assumed that the lag was relatively short before they occurred in the Egyptian record. On the other hand, if they spread out of Egypt, and were passed from hand to hand along the shores of the Indian Ocean, we have no way of telling the speed of such diffusion. But we do know the time span of the occurrence of any characteristic in Egypt within very close limits.

We will now look at the Egyptian pictorial record and see what it shows us about the development of the sail. The most revealing single characteristic of a sail rig is the location of the mast. We can calculate the distance the mast is located from the end of the bow, as a per cent of the overall length of the boat (length between uprights). For the earliest

³³ R. O. Faulkner, 'A Syrian Trading Venture to Egypt', *Journal of Egyptian Archaeology*, xxxiii (1947), 40-47.

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representation of a sail, which might be a hundred years or so before the start of the 1st Dynasty, we find that the mast is 16 per cent of the length back from the bow (FIG. 2). The next oldest representation has the mast 19 per cent from the bow; this is about the start of the 1st Dynasty (FIG. 1). Then we have a long chronological gap until we get to the 4th Dynasty, where we find masts located 28, 30 and 35 per cent from the bow.³⁴ And for the 5th Dynasty we find masts 28, 32-36, 37 and 41 per cent from the bow.³⁵ In the 6th Dynasty we find masts 28, 29, 33, 34, 36, and 45 per cent from the bow.³⁶ And for the 9th Dynasty we have 40 and 41 per cent.³⁷ For the 11th Dynasty we have the famous model hoard from the tomb of Meket-Ra at Thebes which give the following mast locations on seven boats: 38, 38, 40, 41, 42, 42, and 43 per cent.³⁸ In the 12th Dynasty we have 43, 45,³⁹ and 48 per cent.⁴⁰ Then when we reach the 18th Dynasty we arrive at the ultimate, 49 and 50 per cent.⁴¹ Hatshepsut's ships which made the voyage to Punt at this time all have their masts right in the middle of the boats. I have made no attempt to make this survey of the location of Egyptian masts complete; I have used Assmann's figures published in 1913 and those later works which seemed to fill in the picture. But I doubt that a more complete survey would alter the picture much, although it would probably show the deviation from the mean more clearly.

If we plot this data we see how amazingly slow the improvement of the Nile rig was (FIG. 10). Of course the closer the mast is to the middle of the boat, the more the craft can utilize the wind. We do not see any sharp spurts in development, only a slow evolution which took probably at least 2000 years to attain the optimum mast location. If these boats were organic we would say that we had an excellent example of Charles Darwin's principle of natural selection: those boats with masts more towards the centre would have a better chance of survival because they could sail better. But may not some such slow process have been at work naturally? The boatmen realized that a boat with the mast a little farther back sailed better than another with the mast forward. Probably other considerations, such as running rigging, masting, etc., may have prevented the mast from being moved to the centre at once. At any rate the evidence is incontrovertible proof that progress was very, very slow.⁴²

One may ask, what was the technical significance of the movement of the mast towards the centre of the boat? We have said that the craft could utilize the wind better, and now we will explain this in greater detail. A boat with a mast and sail almost in the eyes of the boat, like those shown on the British Museum vases (FIGS. 1 and 2), could certainly only sail directly before the wind. Oh, it could probably steer a few degrees away from the path of the wind, but it could not do much more. Therefore, the wind had to be blowing in exactly the right direction for a sail to be of any use. Otherwise paddles or oars would have to be

³⁴ A. Assmann, 'Die Schiffsbilder', in L. Borchardt, *Das Grabdenkmal des Königs Sahu-re* (Leipzig, 1913), 148.

³⁵ *Ibid.*

³⁶ N. de G. Davies, *op. cit.*, pls. VII, XIX, XX.

³⁷ J. H. Breasted, Jr., *Egyptian Servant Statues* (Washington, 1948), pl. 74, a, b.

³⁸ H. E. Winlock, *Models of Daily Life in Ancient Egypt* (Cambridge, 1955), pls. 70-76, 82. It is interesting to note that four funerary barks have masts 32 to 35 per cent from the bow (pls. 78-81), obviously preserving on these religious craft the older tradition as one might expect.

³⁹ J. H. Breasted, Jr., *op. cit.*, pls. 70, b; 75, b.

⁴⁰ A. Assmann, *op. cit.*, 148.

⁴¹ *Ibid.*

⁴² We mentioned above that the socket (allegedly for a mast) in the Eridu boat was 38 per cent of the length from the bow. Such a location occurs relatively late in Egypt (FIG. 10). Since there were connections between Mesopotamia and Egypt certainly as early as the Gerzean period, this makes us suspect very strongly that the socket was not for a mast.

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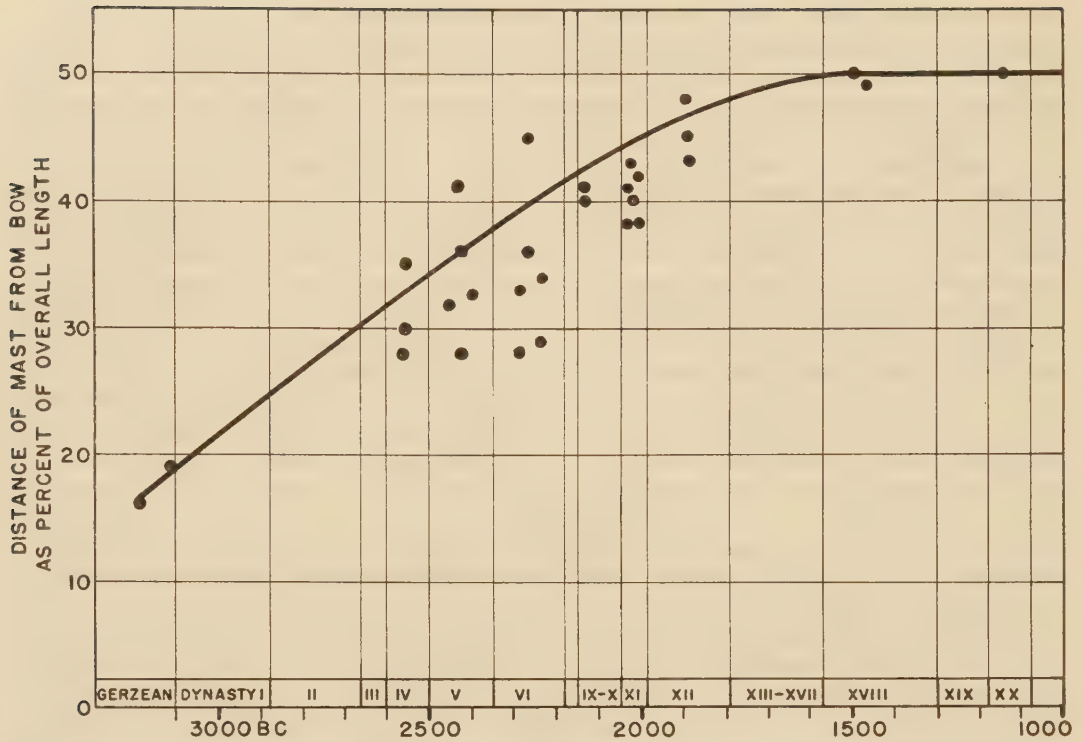


Fig. 10. Graph showing the distance of Egyptian masts from the bow of the boats at various periods of time. The data have been taken from models, reliefs, and paintings. The earliest masts were close to the bow, and it took over a thousand years for the mast to be moved to the centre of the boat. (Original drawing by the author.)

used. In an effort to determine how much boats with masts located like those of the Old Kingdom could use the wind, I rigged an experimental flat-bottomed sail boat with the mast located very closely to that of 5th Dynasty sail boats. The mast location compared favourably to that of Sahure's sea ships (c 2490-2476 B.C.), about 33 per cent from the bow. It was my conclusion after numerous tests that such a rig could only be headed about 30° away from the path of the wind, but certainly not any more.⁴³ Up to this point the boat did not make any leeway with the tall rectangular sail trimmed into the wind. Without the sail trimmed into the wind, the boat could only be sailed 15° away from the path of the wind.⁴⁴ However, it is pretty certain that the Egyptians were able to trim their sails into the wind by the Old Kingdom, if the running rigging were properly used. Actually there would be no sense in moving the mast aft, if they were not trimming their sails into the wind.

If we now go to the ultimate in Egyptian sails and consider those of Queen Hatshepsut's great sea ships of about 1500 B.C., we see an entirely different situation. Here the wide

⁴³ R. L. Bowen, Jr., 'Experimental Nautical Research: Third Millennium B.C. Egyptian Sails,' *Mariner's Mirror*, XLV (1959) 332-7.

⁴⁴ It could still be pointed 30° away from the path of the wind, but it slipped to leeward 15° with the sail set square to the wind.

EGYPT'S EARLIEST SAILING SHIPS

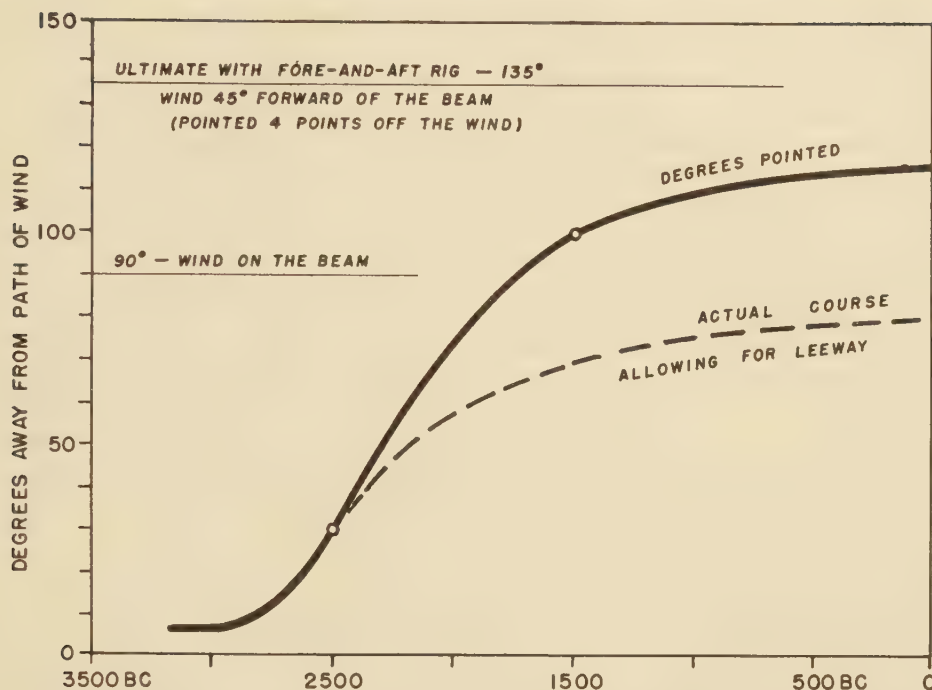


Fig. II. Graph showing the general development of the 'art of sailing' in the Eastern Mediterranean with time. The vertical axis represents the number of degrees which a boat can be pointed away from the downwind path of the wind, and gives a general indication of the advancement in the art of sailing. The dotted line indicates the actual course, taking into consideration the amount of leeway which the flat-bottomed and spoon-shaped Egyptian hulls would make. The hiatus is caused by the introduction of the fore-and-aft rig into the Mediterranean during the last of the 1st millennium B.C. (Original drawing by the author.)

rectangular square sails are set in the middle of the boat. The amount of running rigging on these huge sails makes me believe that the boats could actually be pointed into the wind to a certain extent. This would depend in large degree on the steering gear and how effectively it could keep the boat on a wind. Some of Hatshepsut's ships actually show the mast more than halfway back from the bow, and if true this is very significant, for it would tend to keep the head of the ship into the wind with a minimum of work on the steering gear. On the other hand, the centre of effort of the sail could be shifted aft by taking up the leeward topping lift, which would produce the same effect.⁴⁵ As a guess I would say that Hatshepsut's ships may have been pointed as high as 7 points off the wind (about 80° away from the direction of the wind), or expressed in the opposite manner, 100° away from the path of the wind.⁴⁶ This would be only a couple of points off of the 4 points which a fore-and-aft rig could point into the wind.

⁴⁵ A discussion of some of the running rigging and methods of setting Hatshepsut's sails is to be published in the *Mariner's Mirror* in 1960: R. L. Bowen, Jr., 'Egyptian Sails of the Second Millennium B.C.'.

⁴⁶ The 360° of the compass are divided into 32 points. Square-rigged clipper ships could point as high as five points off the wind.

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However, the amount a sail rig can be pointed into the wind, and the actual course the boat makes are two entirely different things. The determining factor is the design of the hull, which determines the amount of leeway the boat makes when it is on the wind. I sailed an Arab dhow in the Persian Gulf for a number of years. These boats were rigged with large lateen sails which could be trimmed into the wind closely so that the boats pointed a good 4 points off the wind, but they were shallow draught and slipped to leeward about $2\frac{1}{2}$ points, so that the best actual course was $6\frac{1}{2}$ points off the wind.⁴⁷ However, the spoon-shaped hulls of Hatshepsut's ships are another thing. Sølver justly commented that they would drift to leeward 'like haystacks', but he is certainly wrong in thinking that these ships could not sail with the wind more than four points from *dead aft*.⁴⁸ If we assume that they could point at least seven points off the wind, and even if we assume that they slipped to leeward three points, they would be making about 70° away from the path of the wind, which certainly was a lot more than the 30° which the boats of the Old Kingdom could do.

In summary we see that the earliest sailing craft could only steer a few degrees away from the path of the wind. If we assume that Old Kingdom ships of about 2500 B.C. could head 30° away from the path of the wind, that Hatshepsut's ships of 1500 B.C. could head about 100° away, and that a square-rigger of the time of Christ could head 115° away from the path of the wind, we can plot very roughly the development of the art of sailing a square-rigged vessel with time (FIG. 11). This is obviously only a rough estimate based on numerous assumptions. We have also shown in this same graph as a dashed line the actual course boats with Egyptian type hulls would make assuming the leeway they would lose. We see that the development of the true art of sailing, that of using the wind to the fullest degree, was a long and gradual development in the West. And only with a good hull which would hold its course against leeway could the ultimate be attained. There is no evidence that the Egyptians ever had such hulls. But it does seem that they could utilize the wind to a relatively great degree by the 2nd millennium B.C. Sometime in the last of the 1st millennium B.C. the true fore-and-aft sail was introduced into the Mediterranean,⁴⁹ and at this point boats using such a rig could certainly point their craft 4 points off the wind, or 135° away from the path of the wind. Their actual course depended on the design of the hull.

We have attempted to show here that the sail may have been accidentally invented in Egypt. But even if it were invented elsewhere at a much earlier time, the pictorial record shows that the first Egyptian sails were still extremely primitive rigs which could not do much more than sail directly before the wind. The evidence also shows that the first attempts to do more than sail directly before the wind were made at the dawn of civilization at a time corresponding to the early Egyptian dynasties. How much of this development came from outside is not apparent. However, the Nile valley was an ideal location for the development of the art of sailing we actually see unfolding in the Egyptian record. The Nile River flowed north, but the prevailing winds were out of the north. Boats could drift down the Nile, and sail back up against the current with very simple rigs using the prevailing winds. But the Nile does wind to a certain degree, and the wind does not always blow directly up river at any particular point. It may well have been under such conditions that the art of using the wind to a greater and greater degree was first developed. So customary was this means of travelling that the determinative in Egyptian hieroglyphs for 'sail up-

⁴⁷ R. L. Bowen, Jr., 'Arab Dhows of Eastern Arabia', *American Neptune*, ix (1949), 129.

⁴⁸ S. V. Sølver, 'Egyptian Shipping of About 1500 B.C.', *Mariner's Mirror*, xxii (1936), 460.

⁴⁹ See R. L. Bowen, Jr., 'The Origins of Fore-and-Aft Rigs', *American Neptune*, xix (1959), 156-60, 187-8.

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stream ' was a boat with a sail, while that for ' fare downstream ' was a boat with a steering oar, but no sail.⁵⁰

Conditions were diametrically opposite in the land of the Babylonians in southern Mesopotamia. Both the prevailing wind and the river currents moved from north to south. Therefore, it is interesting to note that cuneiform texts indicate that as late as 2100 B.C. the Babylonians sailed *downstream*, but were invariably towed back upstream by man power.⁵¹ These texts also indicate quite clearly that only occasionally when the wind had shifted to the south did Babylonian ships sail upstream. Such conditions certainly did not favour the first developments in the art of sailing such as we see in the Egyptian record. Perhaps in Egypt the necessity for voyages on the open sea, on the Mediterranean and the Red Sea, stimulated the development of the art of sailing, for the Egyptians were certainly sailing out of the Nile and up the Syrian coast as early as the 3rd Dynasty, and probably earlier. But very probably the gradual perfections in sail rig were made in every day sailing on the quiet and sheltered waters of the Nile.

⁵⁰ A. Gardiner, *Egyptian Grammar* (2nd ed., London, 1950), 498, P1, P2.

⁵¹ A. Salonen, *op cit.*, 110, 116-9; A. Salonen, ' Nautica Babyloniaca ', *Studia Orientalia*, XI, 1 (1942), 50.

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Malta and the Mediterranean

by L. BERNABÒ BREA

Dr Bernabò Brea, Director of the Archaeological Museum at Syracuse and Superintendent of Antiquities in Eastern Sicily, here reviews Professor J. D. Evans's volume in the 'Ancient Peoples and Places' series (London: Thames and Hudson, 1959. pp. 256, 35 text-figures, 48 plates, 30s.). He suggests himself a new chronology for prehistoric Malta and argues for the very great importance of Malta in the prehistory of Western Europe, even suggesting that all European Megalithic architecture began there.

PROFESSOR JOHN EVANS's *Malta* was certainly one of the most eagerly awaited of the titles in that fine series 'Ancient Peoples and Places' published by Messrs Thames and Hudson and edited by the Editor of *ANTIQUITY*. This was not only for the exceptional interest of Maltese prehistory to all concerned with the Mediterranean—for it cannot be denied that the architecture of the Maltese temples and the high artistic level of early Maltese sculpture and pottery mark one of the peaks attained by early Mediterranean culture—but because of the experience and attainments of the author, and the important work he has done during his long stay in Malta under the auspices of the Inter-Universities Council for Higher Education in the Colonies.

Already in two important preliminary publications¹ specialists have been given information regarding the conclusions arrived at in this research: these results were of such general interest as to make most desirable a book that would give an up-to-date synthesis of the prehistoric civilization of Malta. Evans, by means of a careful re-examination of all the known material in the light of the results achieved by research into the prehistory of neighbouring Mediterranean lands, and through a series of accurate stratigraphical observations at key sites, has been able to give a clear picture of the succession of cultures and the evolution of styles in the prehistoric pottery of Malta. He has succeeded, too, in linking to the phases of this evolution the various monuments and the various constructional stages in the individual monuments. The development of Maltese megalithic architecture has now left the field of the hypothetical or the uncertain: today we can follow its progress stage by stage.

The two fundamental chapters of the book (which follow a good concise introductory account on 'The Discovery of Malta's Prehistoric Past', and a clear geographical environmental introduction entitled 'The Making of Malta') are devoted to, first, the stylistic and cultural evolution of the great megalithic civilization as determined by pottery development, and secondly to the architecture of the temples and tombs. A third chapter examines the other classes of material whose value is to illuminate the religious, cultural and artistic life of Malta, lingering above all over the artistic manifestation of the highest quality—the sculpture. The last chapter deals with the cultures which flourished in the island after the disappearance of the great megalithic civilization. The volume is illustrated by 35 clear drawings in the text and by fine photographs (among them several air photographs) chosen with great care and excellently reproduced.

¹ *Proc. prehist. Soc.*, 1953, 41, and 1956, 85.

This book marks an enormous progress beyond what was, until a few years ago, the state of our knowledge of early Maltese civilization, and offers for the first time a balanced picture of its development. Concerning the Pleistocene, with its characteristic fauna of dwarf elephants, the problems of the land connection between Sicily and North Africa, and the absence of Palaeolithic cultures, and in its study of the first phase of the Neolithic cultures (the Stentinello-Ghar Dalam phase) the book limits itself to a clear and balanced account without in fact making any great new contributions. The great and important contribution is the discovery of the initial phases of the megalithic culture—that is to say, the stages that preceded the great development of the Tarxien period. These phases, characterized by the styles of pottery decoration which Evans names after Mġarr, Zebbuġ, and Ġgantija, had until recently remained obscure, overshadowed by the splendour of the succeeding Tarxien phase. Most interesting is the argument put forward—which seems to me perfectly reasonable and acceptable—that Maltese megalithic architecture was derived from rock-cut tombs, and its attribution, as a result, to a religion based on an ancestor cult, as well as on that of the Great Mother, Goddess of Fertility.

It is extremely interesting to follow the development of this architecture from the more simple trefoil forms of the little temples of Mġarr I and II and Kordin III to the more complex forms of the great temples in the Ġgantija and Tarxien periods, in which the number of apsidal or transeptal rooms reaches five (or even seven in the middle temples at Tarxien). The temples themselves become enriched by complicated internal structures, and are adorned with reliefs.

Professor Evans's greatest contribution to the understanding of the cultures that follow the disappearance of the Maltese megalithic civilization lies in his study of the Tarxien Cemetery phase, to which, in addition to the cremation graves discovered by Sir Themistocles Zammit, he attributes the 'dolmens' and funerary monuments which he himself excavated at Ta Hammut and Wied Moqbol.² He also refers to this phase the other Maltese 'dolmens' whose chronology until now remained extremely uncertain. He makes interesting comparisons between these Maltese 'domens' and those of the Otranto group in Apulia.

In his evaluation of the Maltese megalithic civilization in relation to Mediterranean prehistory in general, Evans shows great restraint, and in my opinion too great a restraint, in that he minimizes the significance of prehistoric Malta. It seems to me that in the civilization of the west, and hence in the formation of European civilization, Malta had a very decisive function, and one that was probably more important than the role of prehistoric Sicily. It may well be that Professor Evans is reacting to the thesis, pushed so far by Ugolini, that Malta was the primary focus of early Mediterranean civilization. But in so reacting he may fall into an equally exaggerated position, when he describes Malta as in prehistoric times 'turned in on itself, wrapped up in bizarre cults, technologically backward and cut off from the main cultural stream which flowed all about it', and having contact with other regions only to procure for itself the few necessary materials (such as flint, greenstone, obsidian, ochre, etc.) which did not exist locally.

I must admit at once that I am absolutely unable to share this point of view, and that between the two extreme theses, I find myself equidistant. One thing seems to me quite certain, namely that such a splendid culture, artistically so much alive, cannot be simply the result of local evolution favoured by the fertility of the soil and by a completely undisturbed and peaceful farming existence. The conditions which give rise to progress are surely quite otherwise. It seems to me that the flowering of Maltese culture can only be explained, as in the Lipari Islands, as the result of an intensive maritime and commercial

² *Proc. Prehist. Soc.*, 1956, 85.

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activity which presupposes exchanges with both neighbouring and distant lands, and is the natural consequence of Malta's position as an obligatory port of call on all the routes which link the east and west Mediterranean. Indeed, I believe that the cultural superiority of Malta over Sicily is due precisely to the fact that Sicily, agriculturally so much richer, was able to lead a closed life, exploiting its own immense resources, while Malta (again like Lipari) was never able to draw all the necessities of life from its few square miles of sterile and almost waterless rock. Instead, the Maltese of their own initiative had to seek new sources of riches across the seas.

It is obvious that the value of the Maltese contribution to the formation of European civilization depends above all on the dates which we assign to the various stages of the island's culture. The chronology proposed by Evans is very low; he directly equates the apogee of Maltese culture—the Tarxien period—with the early phases of the Mycenaean civilization of which it would be a pale reflection. In my opinion this chronology should be considerably lengthened, indeed by as much as half a millennium. Let us take as a point of departure for our discussion, the later stages to which it is easier to give absolute dates. Pottery identical to that of Borġin-Nadur is found in the cemeteries of the Thapsos culture in the Syracuse area. The Thapsos culture is firmly dated by imported Mycenaean pottery which in some cases seems to go back as far as Mycenaean III A1. The greater part however belongs to Mycenaean III A2, and only a few pieces date from III B. On the basis of these imports we can affirm that the Thapsos culture, whose origins are still obscure, was flourishing before 1400 B.C. and lasted at least into the first part of the 13th century B.C. There is no reason to suppose that the Maltese Borġin-Nadur culture only began a century later, i.e. when the Thapsos culture was dying, especially since in Malta Mycenaean imports of this period are represented at present by only one example. The presence of pottery of the Borġ in-Nadur style (whether produced in Sicily or Malta) right from the earliest Thapsos tombs suggests rather the opposite.

The pottery of the Tarxien cemetery period presents analogies, certainly not accidental, with that of the Capo Graziano culture which precedes the Thapsos-Milazzese horizon at Lipari. The Aeolian culture of Capo Graziano is also very precisely dated by abundant imports of Aegean pottery. Most of this is Proto-Mycenaean but there are certain pieces which could well reach back into the latest phases of Middle Helladic. This indicates that the Capo Graziano culture was already in full flower by about 1600–1550 B.C. But excavations have brought to light levels (and at Casa Lopez on Filicudi a whole village) of this culture which seem to precede the period of Aegean imports. The beginning of the Capo Graziano culture must be earlier than the middle of the 17th century B.C.

It would seem impossible to explain the close parallels between the Tarxien Cemetery pottery and that of Capo Graziano as the effect of a colonization of Malta from the Aeolian Islands, and it is more logical to think of a common origin for the two cultures. Evans believes this is to be sought in peninsular Italy, and points to analogies existing between the Maltese dolmens and those of the Otranto district. But we know what sort of culture existed on the mainland at the time of the Tarxien Cemetery and the Capo Graziano cultures, namely the culture of Gioia del Colle, of Cellino San Marco, Mirabella Eclano, and, on the Tyrrhenian side, that of Gaudio. The parallels between these Italian cultures and Capo Graziano or Tarxien Cemetery are so vague as to exclude the possibility that the island cultures originated here. The origin should rather be sought in the Middle Helladic cultures of the West Peloponnese where Olympia has produced pottery whose forms as well as the motifs and arrangement of the decoration are strikingly similar to the Aeolian and Maltese ones. If the origin of these cultures lies in the Middle Helladic we must admit that the beginning of the Tarxien Cemetery culture precedes the disappearance of this Aegean

culture and must therefore date at latest to the end of the 17th century B.C. and is to all appearances a good deal older still. I should be inclined to date it 1700 B.C. or even earlier.

From these considerations I believe one can be certain that the great Megalithic culture of Malta had already disappeared by 1650 B.C. For this reason it is impossible to see in it the reflections of the Mycenaean civilization. Nor am I able to see in it reflections of Middle Helladic civilization. If one studies carefully the elements which can be paralleled in a general way in the Mycenaean world they would appear rather to be traits which the Mycenaean civilization had inherited from the preceding phases of the Aegean civilization. The Maltese megalithic culture should, in my opinion, be considered as the most westerly outpost of the Aegean culture of the Early Bronze Age, and has close analogies with the Cycladic, Troy I and II, with the cultures of the large islands off the Anatolian coast and with the ancient stages of the civilization of Crete.

For example the very close similarity between the statue menhir from Zebbuġ V and that found by Blegen at Troy I represents no accident nor an isolated phenomenon. The wonderful Maltese statuettes without any doubt have their starting point in Cycladic sculpture, even though they elaborate with striking individuality the Cycladic precepts in the direction of a more lively naturalism. For the origin of the relief art so characteristic of the more evolved Maltese temples it appears unnecessary to follow Evans in seeking its origin among the stelae of the Mycenaean shaft-graves when the pyxides from Melos and Naxos offer even closer comparisons. The figures of ships recently discovered on the upright standing stones of one of the Tarxien temples are most nearly paralleled by those on the decoration of Cycladic 'frying pans'. Many of the Maltese pottery shapes can also be closely paralleled in Cycladic types of the Early Bronze Age.

The comparisons with Middle Minoan Crete which have already been noticed by Sir Arthur Evans and A. Mayr (such as pillar shrines, mushroom altars, the dress of the 'sleeping lady', etc.) certainly bear witness to direct contacts between Malta and Crete in this period and seem to provide a most important chronological reference point. They appear to us to indicate in fact that the Maltese megalithic culture, even though it began during the Early Bronze Age, survived until the beginning of Middle Minoan times and indeed had its most luxuriant growth during precisely this period. Its development, then, would seem to embrace the second half of the 3rd and the first two or three centuries of the 2nd millennium B.C.

Having put back the Maltese megalithic culture into the period where we believe it belongs, the resultant importance of its contribution to the formation of European cultures can be clearly seen. The civilizing of the West Mediterranean, which until now had remained primitive—its transference from the Neolithic to the Bronze Age, the formation of cultures like those of Anghelu Ruju, Almeria and Fontbouïsse, presuppose the civilization of Malta. Malta, far more than Sicily, the Aeolian Islands or the Italian peninsula was the intermediary by which the high technical and cultural condition attained by the peoples of the Aegean and the East Mediterranean was able to take root in the west. I believe that the importance and historic function of Malta in this period cannot be overvalued. One might even ask frankly whether Maltese megalithic architecture is not at the root of all the megalithic architecture of Western Europe. It is a fact that in the Aegean, megalithic architecture does not exist, but we find it widely diffused along all the coasts of the western Mediterranean which received impulses from the Aegean cultures of the Early Bronze Age coming, we believe, by way of Malta.

Evans observes that the characteristics of Maltese rocks and their habit of fracturing naturally into large slabs seem to constitute the most favourable pre-conditions for the birth of megalithic architecture. I must record here my belief that in no part of the Western

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Mediterranean does there exist a manifestation of megalithic architecture which can be referred with any certainty to a period earlier than that to which the Maltese megaliths must be ascribed.

Evans believes that he can see an uninterrupted continuity from the oldest Neolithic (the culture of Stentinello type represented at Ghar Dalam) to the earliest stages of the megalithic culture (represented by the pottery styles of Mgarr and Zebbuġ). The megalithic civilization would, in his view, blossom on a Stentinello stem, from a culture which generically, at least, belongs to the oldest Mediterranean Neolithic—the Neolithic with impressed wares. This interpretation presupposes the very lengthy survival of this culture through all the Middle and Upper Neolithic.

Now in South Italy and Lipari, as in Greece, a second, painted-pottery Neolithic overlies the primitive levels with impressed ware. Lipari clearly demonstrates the succession of, first, two-colour pottery, then three-colour ware without spiral-meander, followed by a painted ware with spiral meanders, and lastly the monochrome red pottery of the Diana style. The Stentinello impressed ware survives only into the first of these phases, that of the pottery painted in two colours. It is absent from the following phase with the three colour ware. The same seems to happen in South Italy. In Sicily things are less clear because, so far, Neolithic sites with painted ware have not yet been discovered, and the various styles characteristic of Lipari and the Italian peninsula are represented only by isolated pieces and sporadic finds. That the Stentinello culture could have lasted later here than elsewhere, even through the phases of three-colour and spiral-meander ware styles, is a theory which I myself put forward some time ago, but which in reality lacks any confirmation. On the contrary it is now well documented that the Diana culture with its red monochrome pottery was present over a large part of the island, not only around Etna, but in all the south-east corner, on the south coast and also in the interior of Sicily. No trace of Stentinello pottery has ever been found associated with pottery of the Diana style, and evidently by this time the world of Stentinello had completely disappeared.

We now know a number of tombs of this Diana culture although only few have been accurately described. The skeletons in these tombs are always found in a crouched position buried within a roughly oval ditch; none of the tombs is an artificial cave or is collectively used. The rock-cut collective tomb appears in Sicily first with the San Cono-Piano Notaro culture, which is already strongly influenced by the Early Bronze Age of the Aegean.

The continuity which Evans argues between his Stentinello type of Neolithic and the megalithic civilization cannot be accepted. It seems to me that there may well have been a considerable time lapse between them—a gap to be filled in by various cultures not yet identified. I believe that the finding of fragments of Stentinello or Diana wares on the site of a megalithic temple merely indicates that the site had been used several centuries before. The most one need infer is that the temple grew up on a site which had already been the seat of some cult in a preceding period and was therefore considered sacred, just as often today a Christian Church continues the cult of an old pagan temple.

In my view the arrival of influences coming from the Early Bronze Age of the Aegean would contain just that element which determined the birth of the megalithic culture. It would explain too the number of parallels and close analogies which at least in pottery forms, can be recognized between Malta and Sicily during the early Metal Age.

The chronological scheme and the correlations we would now propose with neighbouring lands are set out diagrammatically in the table (FIG. 1).

MALTA AND THE MEDITERRANEAN

Absolute dates	Type site	Contemporary cultures in		Greece
		Sicily	Lipari	
	GHAR DALAM	Stentinello	Castellaro vecchio	A
	?	Traces at Paternò, S. Ippolito, M. Pellegrino	Capri/Serra d'Alto	B
	Traces at Mġarr, St Verna, etc.	Diana	Diana	Γ
	MĠARR	S. Cono-Conzo	Piano Conte	Early Cycladic
	ZEBUĠ	Serraferlicchio		
	ĠGANTIJA	S. Ippolito	Piano Quartara	
2100-20000(?)	TARXIEN			M M (Early)
1800-1700 (?)	TARXIEN CEMETERY	Castelluccio	Capo Graziano	M H (late) L H I-II
1400	BORĠIN-NADUR	Thapsos	Milazzese	L H III A-B
1250	BAHRIA	Pantalica	Ausonian I-II	L H III B, III c, etc.

Fig. 1.

Book Chronicle—continued from p. 110

- THE STONE AGE OF NORTHERN AFRICA. By C. B. M. McBurney. pp. 288, 24 plates, 47 text figures. London, Penguin Books, 1960. 6s.
- AN ATLAS OF CHRISTIAN HISTORY. By R. S. DELL. Simple atlas for schools, with introduction, 22 maps and gazetteer. London, George Philip. 4s. 6d.
- SHAFTESBURY AND ITS ABBEY. By LAURA SYDENHAM. pp. 132, with plan, 8 plates and sketch made in 1548. Lingfield, Surrey, The Oakwood Press. 15s.
- BEITRÄGE ZUR CHRONOLOGIE DER URNENFELDERZEIT NÖRDLICH UND SÜDLICH DER ALPEN. By H. MÜLLER-KARPE. Two volumes. Text, pp. 334, 64 figures, 1 map; and volume of 211 plates. Berlin, 1959; Walter de Gruyter (Volume 22 of the Forschungen of the Römisch-Germanische Kommission). D.M. 98.
- GREEK ART AND LITERATURE. 700-530 B.C. By T. B. L. WEBSTER. pp. 125, 8 plates. London, Methuen, 1959. 22s. 6d. Based on the de Carle Lectures given in the University of Otago in 1959.
- MYTH AND SYMBOL IN ANCIENT EGYPT. By R. T. RUNDLE CLARK. pp. 292, 40 figures, 18 plates. London, Thames and Hudson, 1959. 30s.
- ASSYRIAN PALACE RELIEFS. Text by DR. R. D. BARNETT and illustrations selected and photographed by WERNER FORMAN. pp. 36, 173 plates. London, the Batchworth Press Ltd., no date. 42s. A very beautifully illustrated and produced account of the Assyrian Palace Reliefs and their relation to the sculpture of Babylonia and Persia.

Notes and News

REFLECTIONS ON 'THE A B C OF THE BRITISH IRON AGE'

More than a year has passed since the 1958 'Iron Age Conference' organized by the Council for British Archaeology and since the publication of Hawkes's important paper on *The A B C of the British Iron Age*.¹ By now there has been time to study the developed A B C system, to realize its implications, and to test its validity against material from specific sites. The 'New A B C' is complicated and has caused some discomfiture through its rather algebraic approach to an archaeological problem. But the intention of this note is to discuss not the practicability of the 'New A B C', but its validity.

It may seem surprising for a newcomer to question an approach that was generally accepted for thirty years and that has been recently revised and re-acclaimed. But the earlier A B C was sharply criticized by authorities like Daniel² and Wheeler,³ and their objections do not seem to have been answered, but, if anything, emphasized by the 1958 revision. After all, the 'New A B C' is most unusual in its approach: to name all Iron Age groups after a closed series of three letters, and not after an ever-expanding series of type-sites, to define cultural boundaries by fixed geographical limits and not by the concordant distributions of type-fossils, and to fit these groups into an absolute, not a relative framework, all this seems quite contrary to accepted practice and to the methods so successfully employed and so clearly described by Childe.⁴ The A B C scheme implies that the British Iron Age is something apart, not amenable to normal archaeological methods. But is this really so?

There do seem to be quite definite theoretical objections to the scheme. First, the fixed A B C trio unnecessarily limits free interpretation. Fresh material has to fit a preconceived letter category or be forced into one, and each letter implies an automatic interpretation—Hallstatt, La Tène immigrants, Belgae. May Iron Age interpretation never step outside this eternal triangle? Would it not be more objective to keep the description of any group apart from its interpretation? In other words, should it not be possible to refer to a group of material without in the same breath having to announce its ancestry and affiliations?

The rigid geographical limits are a further barrier to progressive interpretation. It is quite possible, for example, that a single intrusive group could stretch out along the coasts of two 'Provinces', but at present it would be automatically split into two and the unity obscured. In similar conditions, could not two distinct groups be erroneously merged into one because they were in the same 'Region' or 'Province' at the same time, even if one were a coastal, the other an inland group? And can we really expect cultural boundaries to remain fixed from the 6th century B.C. to the 1st A.D.?

Then there are chronological problems. British Iron Age dates before the mid-1st century B.C. are extremely tentative, and are at best *termini post quos*. The continental dates on which they depend are themselves far from sure. How, then, can undated British cultures and sites be fitted directly into an absolute framework?⁵

¹ *ANTIQUITY* (1959), 170

² *The Three Ages*, pp. 38 and 43.

³ *Arch. J.*, cvii (1949) Supp., 66.

⁴ For example in *Piecing Together the Past*.

⁵ The division of time into absolute phases with a further series of numbers and letters is an unnecessary complication.

These theoretical objections do seem to be confirmed in practice. A, B and C are 'cultural entities.' If I understand Hawkes correctly, the unifying factor behind A would be a common Hallstatt heritage. Stages within the A culture are then largely defined by the imposition of La Tène features on a non-La Tène background. Long Wittenham, for example, is later First A, 'just touched by the Continental La Tène Culture.' Then comes Second A showing 'internal development' and 'more La Tène influence', footrings, cordons on pottery, etc. It is only after these stages of La Tène influence in the South that sizeable La Tène invasions take place there—Southern First B, characterized by 'pedestal and carinated vases, Swiss bent silver ring and La Tène Ic brooches'. Does this hypothesis really fit the material?

If we consider an early First A site with good associations like West Harling, this classification seems incomprehensible. We should expect to find Hallstatt tradition unmixed with La Tène, but in fact, there are cordons on the pottery,⁶ which should be Second A, and carinated vases,⁷ which should be First B. One sharply carinated bowl with slight beading of the rim, looks almost like a Marnian import.⁸ If we *have* to interpret West Harling by continental standards it means that the only well-excavated and published early First A site that has produced much material is basically La Tène, and not Hallstatt at all.

If we take another well-known, but less well excavated and published early First A site, Eastbourne, there is on the famous vase with painted lozenge decoration⁹ a developed hollow pedestal base, that should be First B, not First A.

If, on the other hand, we look for a Southern First B Culture notably different from Southern A, the evidence for it seems very scattered and hardly convincing.¹⁰

The carinated vases from Worth (a site without associations)¹¹ are just like those from West Harling, and the fibula from this site,¹² is a La Tène Ia type, not Ic. The Ic fibula at Findon Park is linked with pedestal-base urns found in the same pit, but these are devolved, and so not likely to be brought by invaders. The assumption that they are much earlier than the similar 'Wealden' *foot-ring bowls* rests on the date given to this fibula, but La Tène I fibulae are known to remain in use late in Britain.¹³ The unassociated pedestal bases from Park Brow and Worth have a likely First A prototype at Eastbourne. But even without Eastbourne, and supposing that the pottery shows La Tène influence, why should La Tène pottery mean invasions here and not at West Harling? The unassociated bent silver ring is again hardly invasion evidence. In fact, the distinctions drawn between A and B in parts of Southern Britain seem almost imaginary.

Yorkshire, with its chariot burials, is quite distinct culturally and economically from the southern sites just mentioned, but does it fall into a 'cultural entity' with Glastonbury Sutton Walls and the other B groups? Difficulties with C are just as evident, and Brailsford has pointed out the important distinctions between Aylesford C with its pedestal urns and cremations, and the Maiden Castle War Cemetery C with its quite different pottery and burial rite.¹⁴

In view of these theoretical and practical difficulties, might it not be worth trying a fresh

⁶ *P.P.S.*, xix (1953), figs. 10:7 16: 80, 89; pl. III, 9

⁷ *Ibid.*, especially fig. 15.

⁸ *Ibid.*, fig. 15: 69; cf. *Rev. Arch. Est.*, ix (1958), p. 221, groups 8, 25.

⁹ Curwen, *Arch. of Sussex*, pl. xxv, 1.

¹⁰ Hawkes, *Sussex Arch. Coll.*, lxxx (1939), 230.

¹¹ *Ant. J.*, xx (1940), 117.

¹² *Ibid.*, 120.

¹³ cf. Wheeler, *Maiden Castle*, p. 251.

¹⁴ *P.P.S.*, xxiv (1958), 101.

approach to Iron Age classification? Might it not, after all, be both simpler and more objective to define cultural groups in the accepted way, by starting with type-sites and type-fossils and working outwards from these, as Piggott has done for the British Neolithic cultures, that were themselves previously A1, A2 and B? Cultural boundaries could then be defined by the distribution of type-fossils and not by fixed 'Provinces' or 'Regions', and a different pattern for the British Iron Age might emerge.

For example, West Harling might be chosen as one type site. The most distinctive objects from the site are the sharply carinated bowls and situlae. These link it up at once with a series of other sites along the Eastern and Southern coasts—Stiffkey,¹⁵ Esher,¹⁶ Minnis Bay,¹⁷ perhaps Worth,¹⁸ almost certainly Kimmeridge.¹⁹ A development of this tradition may perhaps be seen at Caburn I in the south,²⁰ and Fengate in the East²¹, as well as at Kimmeridge and Esher.

The sites mentioned may form the core of an Early Iron Age complex mainly coastal or near-coastal, that extends over parts of two 'Provinces', Southern and Eastern, and that seems quite distinct from the All Cannings Cross group, also in the Southern Province, but with quite different fine pottery. All Cannings itself would not, perhaps, be a good choice for a type-site to contrast with West Harling, because in the absence of good associations, it is difficult to know what is early and what late at this long-occupied site. But All Cannings does seem to represent the core of a contrasting group,²² perhaps infiltrating gradually north-eastwards along the Icknield Way, as Professor Hawkes has suggested, although not really concerned with Caburn I, Fengate and the rest.

But it is premature to speculate like this. The first step should be to define groups in their own right, without immediately interpreting their significance or encumbering them with continental labels. The ancestry and affiliations of the groups should only be sought after their definition. A second requirement would be some form of relative chronology, but that would have to be discussed at length. Perhaps it is time for another Iron Age Conference?

F. R. HODSON

¹⁵ *Norfolk Arch.*, xxv (1934), 408.

¹⁶ *Ant. J.*, xxvii (1947), 36, especially fig. 16, 6.

¹⁷ *P.P.S.*, ix (1943), 36, fig. 6; 38, fig. 8: 3.

¹⁸ *Op cit.*, 117.

¹⁹ *Arch. J.*, xciii (1936), 215, fig. 4.

²⁰ *Sussex Arch. Colls.*, lxxx (1939), 217.

²¹ *Arch. J.*, c (1943), 188.

²² Not necessarily the only one—Eastbourne is again something different.

(*Professor Hawkes writes*: 'The Editor has kindly shown me Mr Hodson's Note in proof, and promised me space in his next number for a brief reply. I greatly hope it may clear away some of Mr Hodson's misconceptions.')

MORE PENNY WHISTLES

The last number of *ANTIQUITY* contained an illuminating account of penny whistles in ancient times contributed by Mr J. V. S. Megaw to which I should like to add a few supplementary remarks. A marble figurine from an Early Cycladic grave on Keros, indicates that something like the 'Roman tibiae pares', two pipes of equal length and form and tuned to the same pitch, was already current in the Cyclades by 2000 B.C. or earlier.¹ The bronze figurine of L.M. I date in Leyden cannot be identified with any certainty as a piper since the hands have been broken off at the crucial point, but a certain example of a man playing the double pipes (not flutes as so erroneously described by archaeologists who ought to know better) is provided by the painted sarcophagus of L.M. III date from Hagia Triada.²

¹ G. Kochler *Athenische Mitteilungen*, 1884, pp. 156–62, H. Bossert *Alt Kreta* (1929), pls. 18, 20.

² H. T. Bossert, *loc. cit.* pl. 71.

The pipes here were perhaps of unequal length and one was furnished with a funnel-shaped horn at the lower end so that we may perhaps compare them to the Roman 'tibiae impares' which were tuned to different notes and were employed to accompany the *Phormio* by Terence³ whereas the *Andria* of the same poet was accompanied by a double pair of brass pipes (tibiis dextris) and a double pair of treble pipes (tibiis sinistris). There were of course also other pipes in classical times such as the celebrated Pan pipes, and a parallel to the bassoon in the form of the *πλαγίαυλος* or 'tibia obliqua'. The *μόναυλος* or single pipe, however, always remained popular. An excellent account of this has been given by N. B. Bodley in his description of the Auloi of Meroe which he dates 15 B.C.⁴

R. W. HUTCHINSON

³ Compare the preface to the *Phormio* 'Modos fecit Flaccus Claudi tibiis imparibus'.

⁴ *A.J.A.*, 1950, pp. 216-40; see also A. A. Howard 'The Aulos or Tibia', *Harvard Stud. in Class. Phil.*, iv (Boston, 1893).

A NEW NEOLITHIC AXE FACTORY IN CAERNARVONSHIRE*

Location: on the north-east shoulder of Mynydd Rhiw, in the Llyn Peninsula of Caernarvonshire, at 800 ft. above O.D. Nat. Grid. Ref. SH23412991. O.S. 6 in. Caerns. XLIV N.W.

The site on Mynydd Rhiw, though first seen thirty years ago, was not recognized as an axe factory until 1956, when recent gorse-burning had revealed that the banks around a line of three supposed hut-circles were largely composed of flakes of a fine-grained rock, with occasional axe roughouts. A preliminary excavation in 1958, by the Royal Commission on Ancient Monuments in Wales, showed that the hollows were the silted up remains of a quarry for the extraction of axe material from a considerable depth. The 1959 excavation was sponsored by the Prehistoric Society, with the Board of Celtic Studies of the University of Wales, and yielded important information about the site, and about the people who had worked there.

The separate hollows on the surface (including two additional small ones) were all that could be seen of what was in effect a more or less continuous opencast working, following a seam of the desired rock. When one part of the quarry had been exhausted, it was partially filled with debris as the scene of activity moved away, and the remaining hollow was used for shelter. In such a hollow in 1959 the first two of a series of four hearths could be ascribed to this Neolithic squatting occupation, which seems to have continued throughout the working life of the factory.

Enough was learned from one full cross-section of the workings to allow some conjecture as to the method of quarrying. The geological and petrological data show that the axe material is an altered sedimentary rock (hornfels), occurring where a stratum of the local Ordovician shales, 2 ft. 6 in. thick, was so heated by an intrusion of dolerite (below it) that its laminated structure gave way to a homogeneous texture. The resultant rock has flaking qualities nearly approaching those of flint, combined with a greater hardness. There is considerable variation of grain size, but the degree of alteration is consistent throughout. The seam dips into the hillside at about 25°, so that to quarry it from a depth of more than 10 ft. would have meant the removal of an uneconomic amount of overburden—fortunately such of the adjacent shale as had to be removed was very broken. With the attainment of a working depth the seam was followed laterally.

* This will be fully published in the *Proceedings of the Prehistoric Society*.

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How the seam was originally found is a matter for speculation. It could have reached the surface at some point, through the overlying 4-ft. thickness of drift. But the latter had been extensively disturbed at an early stage, presumably to extract the fragments of hornfels which it contained, so that the seam may have been found beneath the drift by chance, and quarrying would have developed from that point. The actual detaching of the rock from its seam was probably by percussion after the removal of the overlying shale; a crowbar and sledgehammer had little effect from the front, and there was no trace of fire-setting.

The archaeological implications of the geological facts are important. Wherever these particular shales were subjected to igneous activity, there may well be rock to be found which is indistinguishable from that quarried at Mynydd Rhiw. Whether prehistoric man found it and used it is another matter, but we should be cautious when interpreting the results of petrological identification of axes in distribution. Only when an artifact is of a type known to have been produced at the factory can it be said with a fair degree of certainty to have come from there. For this reason it is important that a full typological series should be available from a well-sealed primary context at the factory, and such a series would also give some idea of the cultural status of the axe-makers themselves.

The rapid primary filling contained many roughouts of axes rejected during flaking operations around the pit; flakes and other *débris* were left where they fell, thus forming the banks. As soon as the hollow began to be used for shelter, domestic tool forms (scrapers, knives, borers, etc.) were incorporated in the deposit in association with the first two hearths, implying some form of seasonal occupation during the life-span of the factory. These contemporary levels were well sealed over by a sequence of natural weathering layers, ending with the present-day peaty humus, during which the only observed interruption was in the second half of the 12th century B.C. (a date obtained from C-14 analysis), when a group of unidentifiable nomads had used the hollow again for shelter. Around their two successive hearths they left no distinctive material culture.

From the mass of material available for study from the Neolithic levels, certain types can be singled out as of special significance. In general terms, this factory has many things in common with the other factories already known around the Irish Sea (Graig Lwyd, Tievebulliagh, Great Langdale). All were producing tree-felling axes for the Neolithic agriculturalists, but the men who worked them show their Mesolithic, hunter-fisher ancestry in the distinctive Secondary Neolithic domestic tools at the factories. At Mynydd Rhiw were heavy scrapers, knives and choppers made on flakes pre-shaped on the core, even a burin. But the most telling products are adzes and predominantly light and slender axe forms, surely intended for woodworking rather than tree-felling. The few Mynydd Rhiw (Group XXI) axes so far identified inland are all of the heavier type. The probable explanation, which needs much research to support it, is that the lighter and more specialized tools were the economic requirements of the craftsmen themselves. With their familiarity with the sea (to which the distribution of the axe factories bears witness), they may well have needed them for boatbuilding. And if the sea was their medium of transport, perhaps it also provided their subsistence. The carefully made discoidal and lanceolate knives found at Mynydd Rhiw would be in place in such an economy, for flensing and other sea-hunting purposes. In the case of this new factory at least, if not the others, it can be seen that the craftsmen were primarily concerned with their own needs, and that the manufacture and distribution of larger axes was of secondary importance, with the initiative presumably coming from those agriculturalists who required them for their own economy.

C. H. HOULDER

ROMAN FIND AT WELSHPOOL, MONTGOMERYSHIRE

PLATES XV, XVI

In August, 1959, workmen laying a drain in the new cattle-market at Welshpool discovered what appears to have been a Roman funerary deposit similar to those made in the well-known Roman barrows, the Bartlow Hills. The finds comprised bronzework, ironwork, and glass. No bones, cremated or otherwise, nor any form of cist or chamber were noticed: there is in fact a tragic lack of documentation which, it is to be hoped, is not irreparable. The objects were recovered by an amateur archaeologist, Mr G. H. Shepherd, and the National Museum of Wales heard of them through the good offices of Mr J. Bartlett, Director of the Hull Museums. The Welshpool Borough Council, whose property the objects are, has kindly deposited them at the National Museum of Wales for three years, part of which will be taken up with the pressing tasks of conservation.

The bronze objects comprise (a) a ewer, 9 in. high (PLATE XVI (a)) with a handle in the form of a boy, well-modelled in the round, wearing a lion's mask, and standing on one serpent while closely examining a second. The attitude throughout is one of repose and, since the lion's-mask should—by anticipation—identify the figure as the infant Hercules, it may be suggested that he is here shown after the act of strangling the serpents. No parallel to this illustration of the myth is yet forthcoming. Secondly, there is (b) a patera or skillet, 7 in. in diameter, with a central boss and a much-decorated handle, partly inlaid with silver (PLATE XVI (b)). It terminates in a mask of Bacchus and, on the escutcheon, has a profile Bacchic mask similar to those appearing on skillets from the Bartlow Hills and elsewhere. There is also a small human head at the top of the handle above the escutcheon. There are then (c—d) two skillets, 8½ in. in diameter, of more ordinary type, with fluted handles ending in ram's-heads. One has been mended. The last art-object (e) is one of a pair of ox-head escutcheons from a bronze-mounted bucket (PLATE XV): Celtic in style, and among the best of its not very numerous class. A few pieces of the wooden bucket-staves also survive. With the exception of (e) the bronzes seem all to have been packed, wrapped in cloth, in a large spun bronze cauldron with iron-bound rim, of which several fragments were recovered.

The ironwork is in good condition apart from superficial damage, but is so encrusted with rust, etc., as to be virtually unrecognizable until cleaned. There seem to be three large pieces, one of which may be a fire-dog. The glass comprises parts of a square bottle with the letters A F on the base, and a coarse, rimmed jar in greenish-blue thick glass. Both these vessels were smashed by children as they lay on the spoil-heaps. A few scraps of pottery, from two vessels, complete the inventory.

The deposit should belong to the 2nd or 3rd century A.D.: further work may permit greater precision in due course. The presence of this predominantly south-eastern type of burial—as burial it must surely be—in the Welsh marshes is an intriguing one.

GEORGE C. BOON

PACIFIC SCIENCE CONGRESS

The Tenth Pacific Science Congress of the Pacific Science Association will be held in the University of Hawaii, Honolulu, from 21 August to 6 September, 1961. It is sponsored by the National Academy of Sciences, Washington, D.C., and the Bernice P. Bishop Museum, with the co-operation of the University of Hawaii. The Pacific Science Association is an international, regional, non-governmental scientific organization founded in 1920 when the First Pan-Pacific Scientific Conference was held in Honolulu, Section VII of the Congress deals with Anthropology and the Social Sciences. All enquiries should be addressed to *the Secretary General, Tenth Pacific Science Congress, Bishop Museum, Honolulu 17, Hawaii, U.S.A.*

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PLOUGHING EXPERIMENTS WITH A RECONSTRUCTED DONNERUPLUND ARD

Ploughing trials with a full-scale model of the Donneruplund ard were undertaken at the Museum of English Rural Life, Reading, in 1956/7.* Only three short trials were possible but although unsatisfactory in some respects, they do suggest that a more extensive series would be very valuable. The objects of these experiments were: (a) to get the feel of the ard; (b) to discover a suitable means of traction and method of harnessing; (c) to observe the nature of the furrow cut in a limited set of conditions.

The Donneruplund ard (FIG 1 (a)) was chosen because it was, until very recently, the best preserved of all the Danish beam ards, that is of the only type of prehistoric ard so far found in this country.

The plans for constructing the plough were obtained from Dr Axel Steensberg of the National Museum of Denmark. The work was carried out at the University of Reading Museum of English Rural Life with the support of J. W. Y. Higgs, who was then Keeper. The construction of the facsimile plough was undertaken by J. W. Anstee in the Museum workshops.

The Donneruplund ard was found in 1944 and is dated to the middle of the 1st millennium B.C., at the close of the Late Bronze Age or beginning of the Iron Age.¹ It consists of a curved draught-beam 6 ft. 8 in. long with a rectangular slot near the base (FIG. 1 (d)) which is the point of assembly upon which the whole structure of the ard depends. Through this passes a long arrow-shaped share (FIG. 1 (f)) which holds a foreshare (FIG. 1 (e)) between two dowels on its upper surface (FIG. 1 (b)). Both these shares were of oak and the original main share has worn, rounded edges, the wear being slightly greater on the right side than on the left. Beneath them a curved stilt (FIG. 1 (g)) was held in position by a tongue in front of the beam and by a lashing round the tang of the main share (FIG. 1 (c)). The oak of the original stilt was replaced by yew owing to the difficulty of finding a suitable curved branch and, similarly, the birch of the original beam was replaced by apple. The top of the stilt was missing but the reconstructed handle is copied from the Døstrup ard.²

Similar implements are represented in Britain by two finds in Scottish peat-bogs. A complete, but undated, plough beam about 8 ft. 10 in. long was recovered from a peat-bog near Lochmaben, Dumfriesshire, with a draught-hole at the fore-end and a rectangular mortice near the foot for a share and stilt.³ Also, a plough-head and stilt was found beneath a crannog dated to the 2nd century A.D. at Milton Loch, Kirkcudbrightshire, nine miles west of Dumfries. It was 4 ft. 2 in. long with the stilt—in contrast to the Donneruplund or Døstrup examples—quite straight.⁴ The plough-head was grooved, apparently to take a narrow foreshare or a ridged arrow-shaped share like the Trollerup example.

The first trial was in November 1956 on a field from which potatoes had recently been removed, leaving a loose loamy soil. A single horse was harnessed to the ard by means of a swingle-tree, whose hooks were attached to a peg fitted through the hole in the front of

* Our thanks are due to J. W. Y. Higgs, J. W. Anstee, Dr Axel Steensberg, Dr F. Sachs, C. A. Jewell and R. Aitken for their help and suggestions, and to our artist Eric Moss.

¹ P. V. Glob, *Ard og Plov*, 1951, p. 114.

² *Newsletter* 1 and 6, International Secretariat for Research on the History of Agricultural Implements. A more complete plough of the Døstrup type was found in 1957 at Hendriksmore, Jutland, Denmark, and dated by C-14 analysis to 350 B.C. (± 100 years).

³ *Agricultural History Review*, vol. v, pt. II, 1957, p. 74., and information from Mr A. E. Truckell, Dumfries Museum.

⁴ *Proc. Soc. Ants. (Scot.)*, LXXVII, 1952-3, pp. 143-4 and pl. xvi.

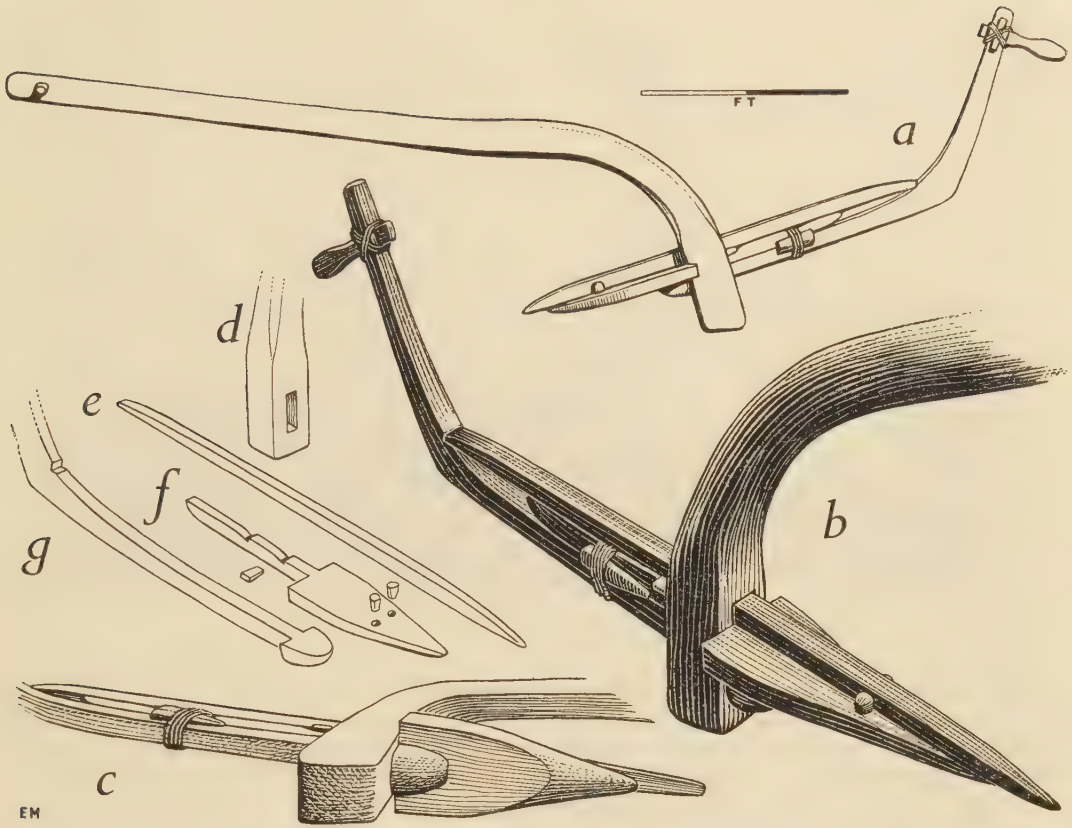


Fig. 1. (a) Profile of the reconstructed plough. (b) View of the fore-share resting on the main share between two dowels. (c) The underside of the beam and shares, showing the position of the tongue of the stilt. (d) The rectangular slot at the base of the beam. (e) The long narrow fore-share. (f) The main arrow-shaped share, showing the dowels inserted in the blade and a wedge used to tighten the shares in the beam. (g) The lower part of the curved stilt, showing the tongue to hold it in the beam, and the nick in which one end of the fore-share rests.

the beam, the overall length of plough and horse being 18 ft. Prehistoric rock drawings, such as that at Aspeberg, Bohuslan,⁵ show that the original draught team for this type of plough was normally a pair of oxen, though it may have varied.⁶ To increase its life a small iron tip was fitted over the front of the fore-share for this and the second trial. Ploughing (FIG. 2) lasted for an hour. The horse proved unsteady and due to the irregular draught, loose soil and uneven pressure on the stilt, the furrows varied considerably. The ard cut a V-shaped furrow generally 10-12 in. wide and 6 in. deep, but it sometimes dug in to a depth of 9 in. In the loose soil it was also noted that the earth compacted before the beam, filling the angles between the fore-share and the blade of the arrow-shaped share to form a smooth convex surface.

⁵ P. V. Glob., *op. cit.*, p. 46.

⁶ F. G. Payne, 'The Plough in Ancient Britain', *Arch. J.*, CIV, 1947.



Fig. 2. The Donneruplund ard in use, November 1956.

A second attempt to use the plough was made in April 1957, when a tractor replaced the horse in an endeavour to obtain a steadier draught. The beam of the plough was loosely hitched to the draw-bar of the tractor, which was driven slowly at approximately 3 m.p.h. The field used was under stubble, the soil being a rather stony gravel, and an hour and a half's ploughing took place before the shares broke. The ard cut up the stubble very easily if heavy pressure was maintained on the stilt, this time to keep the share in the ground. When the ard was tilted, always to the right during the trials, it was often possible to turn a furrow slice up to 7 in. wide. However, this could not be accomplished consistently, and in attempting to do so with the plough leaning at an angle of 30° from the vertical the shares broke. An examination of the broken parts suggest that the arrow-shaped share split first, followed by the foreshare, which was left unsupported. The underlying reason for the breakage was bad wood in the arrow-shaped share, but the tilting of the plough was also a factor. The tractor did not prove very satisfactory as a source of power since it was difficult to cut close parallel furrows. Also the front end of the beam did not have the same freedom of movement as when draught animals are used.

In the third trial, held in June, 1957, the ard was used for half an hour. New shares were fitted but an iron tip was not used on the fore-share. The soil conditions were the same as during the second trial except for a considerable hardening of the ground due to a long dry spell of weather. The horse was again used, and proved more amenable and steadier than on the first occasion. Despite heavy pressure on the stilt the share would not dig in and merely skidded across the ground. This was partly due to the hardness of the ground, but was probably also connected with faulty handling whereby too much reliance was placed on downward rather than forward thrust.

Tillage experiments with reconstructed ploughing implements have also been carried out at the Czechoslovak Academy of Agricultural Science by Dr F. Sachs. A variety of ploughs were tried on different soils for comparative efficiency, and these tests showed

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like those at the Museum of English Rural Life the importance of the harness, traction power and experience of the ploughman.

In sum, then, valuable lessons were learned on the handling and harnessing of the ard, but technique must be further improved before the very tentative observations on its performance can be verified. It is felt that future experiments should be directed towards the following ends:

- (1) The reasons for cross ploughing (difficulty of making close parallel furrows ?);
- (2) whether this ard can consistently turn a furrow, and if so, whether it can build a ridge ?;
- (3) the time needed to build a lynchet on different slopes and soils;
- (4) the difficulties involved in, and the effect of, ploughing an established ' Celtic ' field.

F. A. ABERG and H. C. BOWEN

C-14 NEOLITHIC DATES FROM FRANCE

The readers of *ANTIQUITY* already have heard about some radio-carbon dates from Dr P. R. Giot's excavations in Brittany, established in Groningen by the late Professor Hessel de Vries.¹ Some other dates of Neolithic sites of France have recently been produced by the Laboratoire du Radiocarbone, Gif-sur-Yvette, of which Professor J. Coursaget is director, M. J. Le Run chief engineer, and Dr P. R. Giot archaeological adviser. This Laboratory is a joint plant between the Centre National de la Recherche Scientifique and the Commissariat à l'Energie Atomique.

The Laboratoire du Radiocarbone, Gif-sur-Yvette, has recently dated a series of Neolithic sites from different parts of France, which bring forward an interesting contribution to the absolute and even relative chronology of the Neolithic cultures of that part of Europe, and that also agree with the general tendency that is shown by the mass of radio-carbon dates coming from the different laboratories, inducing a ' long ' chronology instead of a ' short ' one as was in favour in many archaeological circles up to recently.

Thus the important stratified site of Roucadour, Thémines (Lot), excavated by the late A. Niederlender,² has given three dates:

The strata C, with so-called ' impressed pottery ', 3980 ± 150 B.C.;

The strata B₁, with so-called earlier ' *chasséen* ', 3230 ± 140 B.C.;

The strata B₂, with so-called later ' *chasséen* ', 2320 ± 125 B.C.

In the same district, from the site of the cave of the Perte du Cros, Saillac (Lot), excavated by the Abbé Galan,³ the hearth no. 3, with so-called ' *chasséen* ' pottery, has been dated twice: 2165 ± 125 and 2330 ± 125 B.C., giving a mean date of 2250 B.C.

In Normandy, from a habitation hearth on the important site of the Mont-Joly or La-

¹ *ANTIQUITY*, xxxiii, 1959, p. 238, quoting P. R. Giot, *Bulletin de la Société Préhistorique Française*, lvi, 1959, p. 292-3. A full paper is in press in *Bulletin de la Société Préhistorique Française*, lvii, 1960. The Neolithic settlement of Curnic, *Guissény* (Finistère) is dated 3140 ± 60 B.C.; Neolithic influence on vegetation is attested before 3210 ± 60 B.C. in the palynological sequence of the Yeun-Elez, Brasparts (Finistère), studied by Dr W. Van Zeist. The Passage Grave of Ile-Carn, Ploudalmezeau (Finistère), with a tholos corbelled structure, is dated 3030 ± 75 B.C. (about this monument, cf.: *Bulletin de la Société Archéologique du Finistère*, lxxxi, 1955, p. 53-62; *Gallia*, xii, 1954, p. 403-4). See also a general paper by P. R. Giot, *Annales de Bretagne*, lxvi, in the press.

² A. Niederlender, R. Lacam and J. Arnal, *Bulletin de la Société Préhistorique Française*, xlix, 1952, p. 477-9; L, 1953, p. 241-8 and 515-7. A. Niederlender, *Bulletin de la Société d'Etudes du Lot*, 1954. J. Arnal and Cl. Burnez, 37-38 *Bericht der Römisch-Germanischen Kommission*, 1956-7, *passim*, p. 1-36, pl. 1.

³ A. Galan and H. Bessac, *Bulletin de la Société Préhistorique Française*, lii, 1955, p. 669-70; *Zephyrus*, vi, 1955, p. 285-92. A. Galan and J. Arnal, *Bulletin de la Société Préhistorique Française*, liii, 1956, p. 255-61.

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Brèche-au-Diable, Soumont-Saint-Quentin (Calvados), excavated by B. Edeine,⁴ with sherds of 'chasséen' pottery looking like that of Fort-Harrouard I, a double dating has been produced, 2800 ± 130 and 2865 ± 130 B.C., giving a mean date of 2830 B.C.

In Central Western France, the camp of Les Matignons, Juillac-Le-Coq (Charente), excavated by Cl. Burnez,⁵ has shown in its inner ditch a stratigraphical sequence. A burnt layer indicating the end of the occupation of the camp by its 'chasséen' builders has been doubly dated, 2505 ± 160 and 2725 ± 160 B.C., giving a mean date of 2615 B.C.

In Brittany, the barrow of Kermené, Guidel (Morbihan), excavated by P. R. Giot,⁶ with Late Neolithic pottery of S.O.M. affinities and with fragments of a broken statue-menhir, has been dated 2070 ± 110 .

Two sites of the 'Chalcolithique' of the Grands Causses, excavated by L. Balsan,⁷ have been dated. The first, the ossuary of the Côtes-de-Roquefort, Saint-Rome-de-Cernon (Aveyron), with two datings, 2015 ± 125 and 1930 ± 125 B.C., giving a mean of 1970 B.C. The other, the Aven-du-Gendarme, La-Roque-Sainte-Marquerite (Aveyron), with two datings, 1945 ± 125 and 1895 ± 125 B.C., with a mean date of 1930 B.C.

With the dates established by other Laboratories, either from Brittany,¹ from the so-called late Danubian of the Paris Basin,⁸ and from a few other sites, together with all the chronological information of the same sort from the other countries, we have now quite a fine chronological framework, being continually corrected and completed by the more and more greater output of dates from the different laboratories, every month bringing in more information.

J. COURSAGET, P. R. GIOT, J. LE RUN

⁴ B. Edeine, *Bulletin de la Société Préhistorique Française*, LVII, 1960, in the press; *ibid.*, LIV, 1957, p. 60-2.

⁵ Cl. Burnez, M. Drion, T. Gee and Ph. Grafe, *Bulletins et Mémoires de la Société Archéologique et Historique de la Charente*, 1958.

⁶ P. R. Giot, *Annales de Bretagne*, LXV, 1959, p. 5-30; *Gallia-Préhistoire*, III, 1960, in the press.

⁷ L. Balsan, *Gallia*, VI, 1948, p. 404-5.

⁸ H. Jouillé, *Bulletin de la Société Préhistorique Française*, LVI, 1959, p. 479-85. A pit with so-called 'Late Danubian' (*Rubanné récent*) at Vailly-sur-Aisne (Aisne), dated 3440 ± 300 B.C. by the *Service d'Electronique Physique* of the *Centre d'Etudes Nucléaires*, Saclay.

ASWAN AND UNESCO

We printed in the last number of ANTIQUITY a note from Professor Plumley on the Aswan High Dam project and archaeology (ANTIQUITY, 1960, 62), and since then, on 8 March, Unesco launched its great international campaign to save and explore the antiquities of Nubia. Many archaeologists are still anxious about the successful outcome of this campaign which will need the services of many trained archaeologists, perhaps seconded from other fields than Egyptology. In a recent broadcast in the B.B.C.'s Arabic Service, L. P. Kirwan emphasizes the urgency of the problem and says 'in terms of actual working time, twelve months, eighteen months perhaps, will be all the time left to save Nubia's past from the flood' (*The Listener*, 17 March, 1960, 492). Professor W. B. Emery, Head of the Department of Egyptology, University College, London, is a member of the International Consultative Committee set up by Unesco to deal with the problem of the threatened antiquities, and has promised to write for the next number of ANTIQUITY a note on the Committee's plans and programmes and on the proposed recruitment of archaeologists. Meanwhile readers are recommended to read the special February 1960 number of *The Unesco Courier* (2s. 6d.) entitled 'Save the Treasures of Nubia' and the articles in *The Illustrated London News* for 9 April and 23 April, 1960.

Reviews

OLD AFRICA REDISCOVERED. By BASIL DAVIDSON. *Gollancz*, 1959. pp. 268. *Reference and Index*. 25s.

‘ Where once the porcelain stood in the wall niches
Now wild birds nestle their fledglings ’

is one of the well selected quotations with which Basil Davidson has introduced his chapters; and the early 19th century Swahili poet to whom it is attributed would clearly have had some regard for the task which many of those who have helped Mr Davidson are undertaking. This is not quite the first popular general book on the earlier African past—there is the Ghanaian de Graft Johnson’s *African Glory* (1954), written with something of the same intention—but it is a sign of the very rapid growth of interest in African archaeology and history in the last decade, a growth which the African History Conference of 1953 both pointed and stimulated.

Though, of course, Old Africa has not been without earlier enthusiasts. There are the uneven reports of medieval and premedieval writers, represented for West Africa in Bovill’s *Caravans of the Old Sahara* (1933), and for the East African coast, conveniently and briefly reviewed in Dr Freeman-Grenville’s paper, ‘The Times of Ignorance’ (Uganda Museum Occasional Paper No. 4, 1959). There is the literature represented in Pinkerton’s *Voyages and Travels*—how informative is much of Bosman’s *The Coast of Guinea*, or the sympathetic Abbé Proyart’s *History of Loango*; ‘... in this history we shall hazard no conjecture ... it is not to be expected that we should give a very extensive account of the history of nations who, as yet, have neither acquired the use of letters, nor employ any substitute for them.’ If the modern advances in archaeology and the sifting of oral traditions have proved the Abbé somewhat too cautious, that was a fault on the side of intellectual virtue.

Nearer to the work now in progress are the more specialized and pioneer studies of half a century ago—one thinks at random of Ling Roth’s *Great Benin* (1903) with its emphasis upon the African characteristics of Benin art; or Randall MacIver’s *Mediaeval Rhodesia* (1906), with its hope for a combination of ethnological and archaeological studies that even now has scarcely started to be realized:

‘ A corner is lifted from that veil which has shrouded the forgotten but not irrecoverable past of the African negro. Were I a Rhodesian I should feel that in studying the contemporary native in order to unravel the story of the ruins I had a task as romantic as any student could desire.’

And there is the interest in the African past professed by Sir Hugh Clifford in his introduction to Claridge’s *A History of the Gold Coast and Ashanti* (1915), or by General Wingate in his encouragement of the first *Sudan Notes and Records* (1918), an interest which that distinguished journal has continued to nourish over many years.

Not everyone, then, has been blinded by the anti-African (or more precisely anti-negro) prejudice that Mr Davidson rightly, if repetitively, refers to. Something was attempted, even before the independence achieved by many African states gave a special urgency and direction to the exploration of their pre-colonial past. Nevertheless it is true, as Mr Davidson suggests, that until recently African studies have tended in general to be regarded as the poor relations of ‘respectable’ scholarship, partly because of the absence of any single

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ancient literary language for most of the continent, but partly also as a result of a vulgar disvaluing of African cultures accompanying certain kinds of European self-complacency.

It is against this background that popular appraisals of the African past (and there are bound to be more of them) must for the time be understood; and it is for people who initially find it difficult to take African culture seriously that Mr Davidson's book is intended—for those, for example, who provoked Aimé Césaire, one of the best known poets of the movement of negro intellectuals represented by *Présence Africaine* to make, if ironically, a real virtue out of the deficiencies of which Africa is sometimes even now accused:

Eia pour ceux qui n'ont jamais rien inventé
pour ceux qui n'ont jamais rien exploré
pour ceux qui n'ont jamais rien dompté
mais ils s'abandonnent, saisis, à l'essence
de toute chose

This, one may reflect when reading part of *Old Africa Rediscovered*, is a healthy reaction against any tendency to make the discovery or conjecturing of an early history, and pre-history, a condition everywhere of African national self-respect. For if it becomes so (and there are one or two signs) standards of scholarship are bound to decline, and just when the increasing body of factual information would permit them to become more rigorous. In earlier times, the African rediscovery of an African past was indeed part of a process of rehabilitating African cultures among others of the world. As early as 1911 the farsighted Ghanaian, Casely Hayford, was proposing an African National University, with a Professor of History paying special attention to the African past. Now, with Fage's *Atlas of African History* (1958) published, a *Journal of African History* and several early histories of parts of Africa announced, and an increasing programme of excavation and investigation—and these are but a few indications of what is happening—the subject of Mr Davidson's book is established as academically satisfying in its own right. It should need to draw less and less upon inspirations which have little to do with the cold examination of evidence.

'If a man will begin with certainties he shall end in doubts: but if he will be content to begin with doubts he shall end in certainties' is a scientific maxim of Francis Bacon which prefaces Mr Davidson's survey; but in covering such a vast area, and trying to give some *connected* account of a past that, in truth, is as yet lit up only here and there by certain knowledge, it has been difficult to avoid introducing purely theoretical 'peoples' to fill some of the historically empty gaps and periods of the continent. 'Even the dense forests that wall the Congo witnessed this onward penetration of nameless tribes in times beyond memory. They moved like the unseen armies of the stars . . . in hidden orbits that we do not know.' Mr Davidson fortunately does not need such literary embellishments when his facts are adequate; but here, surely, the distinction between doubts and certainties is obscured.

Mr Davidson has been faced with a problem that will continue for some time in African studies—that of relating the past to the present. Sometimes he seizes upon parallels between ancient and modern custom, and here he could be more exact if he knew more of modern ethnology. (The same might have been said of the late Henri Frankfort, whose *Kingship and the Gods* promised an imaginative development in comparing the ancient and the modern). Sometimes he emphasizes the contrast between past and present, a corollary of his just insistence upon the harm done to some African cultures by slaving, exploitation and war. We are all in this position, for the facts themselves are confusing, and developments have not been uniform over Africa. Yet the most profitable approach, and that now most

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generally being followed, is to proceed as it were from both ends of the time scale, and systematically, in particular areas. On the one side are archaeological discoveries and in some cases early documents. On the other are living peoples, whose own traditions have often not been broken for several hundred years, as far as we can see. Casual parallels, such as that cited by Mr Davidson between the divine kingship of the modern Shilluk and the Zanj of al-Mas'udi, do not take us very far. What is required is the attempt to join up the archaeological evidence with living tradition, and here, for much of Africa, the last four or five hundred years still require much reconstruction, of the kind that Dr Oliver and others, for example, are attempting in Uganda. It is too sweeping, and certainly a counsel of despair, to write that 'the peopling of continental Africa over the past 1,500 years or so was seldom or never with the peoples who are known today.' There are difficulties still, it is true, in establishing continuities at Zimbabwe; but on the coast of East Africa, in Uganda, and certainly in Nigeria and Ghana, more continuity has been preserved than one might suppose from such a statement. They can only be perceived, however, by those with a profound local knowledge of the present and the recent past, and this will increasingly be found in sufficient depth only among Africans, or at least good speakers of vernacular languages. As yet unpublished work on the history of the Akan peoples by Nana Nketsia IV will indicate what can be done in this direction, and more generally, it ought to be admitted that the time has come when real progress in the study of African history may demand a real knowledge of languages still spoken. As for local knowledge, where is the archaeologist or ethno-historian who at leisure, and without mechanical transport, will be able to travel over the suggested routes of migrations of peoples and cultures—Axum, Meroe, Darfur, Lake Chad for example? Who will try to 'sweep' on foot, as so many conquering tribes are supposed to have 'swept', from one territory into another? Mr Davidson did attempt to see the places of which he writes, and his book is the livelier for it.

To say all this is not indeed to criticize Mr Davidson; on the contrary, his work presents (and with great modesty) a survey of the problems and the knowledge which now concern specialists in many African fields, and performs a valuable service in mediating between them. His reference to work done on East African coinage, for example, prompts one to wonder whether any comparable work has been attempted on imported coins in West Africa, where the long history of trade surely suggests that important finds may be made. His survey of the literature has been, for a book of this kind, most serious and thorough, and any interested reader will be able to form from it a just impression of the gaps, both geographical and chronological, in present knowledge. He has taken, chapter by chapter, the main regions of Africa now being studied, and if the total presentation appears rather lacking in continuity despite literary effort, that is because those regions are not as yet in themselves at all certainly connected. It is still difficult indeed to see Africa as a whole, except perhaps in political terms, and anyone who attempts to do so must appear to jump from place to place and period to period. Once out of the palaeolithic, we lose any broad overall picture, and move from site to site—Nok, Meroe, Zimbabwe, Gedi—much as if, in European studies, our route lay undefined between such fixed points as Stonehenge, Venice and Lascaux. In such a situation, any attempt to produce a historical sense of proportion is as welcome as it is difficult.

And here Mr Davidson and those whose work he has used and generously acknowledged begin to produce a kind of guide and sketch-map. The greatest single difficulty, naturally, is that of chronology. Mr Davidson begins excitingly with the story of how Torday dated one of a list of kings of the Bushongo, by an eclipse of the sun which was remembered to have occurred during his reign—the only one visible in the region in the 17th and 18th centuries. Such evidence, however, is rare, and the greatest caution has to be exercised in

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the estimation of dates from royal genealogies. It is a little naïve, for example, for Mr Davidson to remark that 'the Shilluk are rather a new nation' because the king in 1945 was only the 31st of his line.

Such difficulties are far from being solved over the greater part of the continent south of the Sahara. It would appear that the East African coast, with its literary sources (though the cities mentioned in the *Periplus of the Erythraean Sea* have not as yet been identified), its long settlement and external contacts, its porcelain and coins, is the field in which the study of chronology is likely to be most advanced; and if, as is hoped, it can in some way be linked with discoveries in Uganda, or definitely proved not to have been so linked, a little more certainly about scale and distance will be introduced into our map. The discoveries made by Shinnie and others, of Iron Age culture in Uganda apparently antedating the separation of the Interlacustrine kingdoms, and further excavations in the Belgian Congo, may prove to be among the most significant of the last decade from the point of view of consolidation of knowledge; though in richness of material, East and Central Africa still cannot compare with the West. Again, contacts of Uganda with the north are suggested by linguistic evidence (some of the vocabulary of the Banyoro is Nilotic) and by some traditions, but the evidence is slight. Excavation of the mounds in the Bahr al Ghazal Province of the Southern Sudan might be a rewarding task. Meroe is of course remarkable; but any distant southern and western contacts it may have had are still more in the realm of possibility than of fact. Then again (as compared with West Africa) Ethiopia, which must be of vast importance, is little known.

At last, however, we do begin to get an interest in and knowledge of the African past which goes beyond remarking 'strange' parallels between Africa and ancient Egypt, and does not introduce those mysterious 'peoples of Hamitic stock' wherever anything remarkable is found in negro Africa. The next Conference of African History (it is to be hoped that the proceedings of the 1957 conference will be published quickly) will have much to add to what was known at the first, which after all was only seven years ago. The Sudan, Nigeria, and now Uganda and Tanganyika, show what can be accomplished when Antiquities Services receive the official support they deserve, and where the study of living peoples accompanies the study of the archaeological record. In Africa, at least, a good curator of antiquities will never be able to be a simple antiquary.

GODFREY LIENHARDT

ANCIENT MEXICO. By FREDERICK PETERSON. *Allen and Unwin*, 1959. pp. 313, 93 figures, 6 maps, 24 plates. 35s.

No general book on Mexican archaeology has been published since Vaillant's *Aztecs of Mexico* and an up-to-date one is badly needed. Mr Peterson is well qualified to undertake the task; he has lived in Mexico for several years, he understands its atmosphere, and his knowledge is up to date, as is shown by his mention of some recent discoveries which are not generally known. Two of these are important and their full publication will be eagerly awaited; they are a site at San Vicente Chicoloapan in the Valley of Mexico, which may give some substance to de Terra's Chalco stone industry (p. 25), and four new pyramids of the late Formative Period near the famous one at Cuicuilco.

The book begins with five chapters which summarize the chronology from early man to the Spanish conquest. The remaining eleven chapters deal generally with specific features of Mexican culture, including, among others, religion, education, warfare, pottery and art. In all, it contains material for an excellent account of the whole, but I have two general criticisms. The first is that the arrangement tends to obscure the differences between the Classic and Post-Classic Periods, and the great changes, particularly among the Maya,

which most of us believe to have begun with the arrival of the Toltecs at the beginning of the Post-Classic. This may be partly deliberate, since it is stated (p. 151) that there is evidence for large-scale warfare in the Classic, but I hasten to add that the indefensible nature of Classic sites renders this most improbable. My other criticism is that although the book is 'intended as a general introduction . . . with the hope of arousing interest', it sometimes lapses into language or goes into detail which caters rather for specialists. For example, the general reader may well wonder (p. 43) what *petates* and *metates* are, and he is more than likely to be puzzled by the brief references to negative painting of pottery (p. 255, etc.). Such cases could be remedied by means of a short glossary. I doubt if the interest of the same sort of reader will be aroused, for example, by the bare summary of the five Pre-Classic epochs of Chiapas (pp. 49, 50) ('Chiapa III has hard fired white pottery plus batik painting'), or by the unillustrated list of local Maya architectural styles (pp. 288 ff.). The rather loose use of the word city for a ceremonial centre, and of empire for small states (p. 74), and indeed for the Aztecs, is deceptive, but the author is far from unique in this.

There are several specific matters which provoke comment, and I will select a few. In the first chapter, on Early Man, the existence of the Bering Strait land bridge at the relevant time, is more than a theory (p. 19), because we know that there was one from about 25,000 to between 8,000 and 9,000 B.C. The discovery of stone points in the Istápan mammoth skeleton shows that man killed it, but does not clinch the argument about the authenticity of the Tepexpan human skeleton (p. 24). In fairness I should say at this point that the remarks about elephant hunters, which have called down wrath on the author's head, are due to an incautious use of popular language and not to a corrupt following of Elliot Smith, since the context shows that he understands that the creatures concerned were Imperial Mammoths. The remark in the second chapter (p. 36) about the 'terrific desire for expression' of the makers of figurines, together with some comparisons between Mexican and modern art in Chapter XV, should be read in conjunction with the qualification (p. 278) that 'Criticism is still unsure'. Chapter IV, which covers a good deal of the Post-Classic, includes an account of the Chichimecs, which some may find confusing, but it is just to comment that this reflects a very confused situation; the author cites Wigberto Jimenez Moreno as his authority but does not give a reference to him in his bibliography. It is hardly reasonable to say (p. 65) that this period is best known for its use of metals, since they were only used on a very limited scale in the early part of it, and as is stated later (p. 264) 'Mesoamerica was to all practical purposes in a Neolithic Age, as metal was rarely used as tools'. Chapter IX includes a useful and pleasant account of markets, in which the author's knowledge of modern Mexico doubtless stood him in good stead. In 'Numbers and Time Counting', Chapter X, fig. 51, there is a misprint of 2259 against a Maya number of 2258. In the same chapter (p. 190) the statement at the end of the second paragraph that 'all the day names come 5 days later in the following year' is badly expressed and would undoubtedly mislead anyone not familiar with the subject. What is meant is that the day on which a year begins is 5 days later in the sequence than the day on which its predecessor did, so that a year beginning with a day Eb (cf. Table V) will be followed by one beginning with a day Caban. Chapter XIII, 'Writing and Painting', contains a very handy list of the picture books, generally called Codices, but in speaking of wall paintings, having said (p. 245) that it was uncertain whether they were done in 'al fresco' or 'al secco' technique, the author should not have gone on to call them, in several places, frescoes. Still less should he have pandered to Guatemalan pretensions by saying that Santa Rita, British Honduras, was in Belice!

Finally, I cannot resist quoting a plum from the chapter on pottery (p. 257) 'It follows the fads and fashions of the times and reflects the activities and tastes of man. Who recalls

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the times when we made statues of Venus with clocks in their stomachs, or large white spittoons for the tobacco-chewing gentlemen in the barber's shop, or the large white receptacles that were pushed under the bed within easy reach at night? All these reflected our times and were part of our daily life, but as our times change so does the pottery.' As a back number who eats his dinner off plates well over a century old, I can assure Mr Peterson that 'large white receptacles' are still part of our daily life!

This is quite a useful book, but with a little more care and a slightly different slant, it could have been so much better.

G. H. S. BUSHNELL

VRÅKULTUREN, STENÅLDERSBOPLATSERNA VID MOGETORP, ÖSTRA VRÅ OCH BROKVARN. By STEN FLORIN with contributions by MAJ-BRITT FLORIN and ELISABETH SCHIEMANN. *Kungl. Vitterhets Historie och Antikvitets Akademien, Stockholm, 1958. pp. 300, 96 figures and 52 plates (including numerous plans and tables). 100 Swedish kronor.*

This is the definitive publication of the important Vrå Culture in East Central Sweden, known hitherto only from Florin's tantalizing interim report of 1938.

Mogetorp is the earliest, Ö. Vrå slightly later and Brokvarn the latest of these Corded Beaker settlements, yielding Beckers Group C funnel beakers, polygonal battle-axes, thin- and point-buttressed axes, querns and rubbers. The squarish hut foundations on two of the sites are interesting, but not wholly convincing. Most spectacular are the indications of agriculture dealt with in separate sections in German and English: grain and seed impressions of cereals (mainly naked barley but also various wheat types), leguminosae including pea and (probably) bean, vine, and apple, and pollen spectra showing the now familiar pattern of forest clearance for cultivation. Faunal remains, unfortunately, did not survive. A dilemma of a kind now familiar to readers of ANTIQUITY is presented by the C-14 'dates' for Mogetorp—3403, 3373 and 3252 years B.C., all ± 100 . Values of a similar order for equivalent archaeological and pollen stages are cited in support, though the Muld. I dates, for example, could have been used to counter them. Florin accepts these 'remarkably high' values and with them an Early Neolithic of 'at least 7-800 and perhaps 1000 years' duration—surely an '*expansio ad absurdum*'?

The detailed descriptions of the geology, topography, stratigraphy and material from each site, and of the phosphate surveys that proved enviably successful, are models of their kind and are lavishly illustrated with photographs, sections, maps and magnificent photo-gravure plates (and one colour plate). The cost, however, though doubtless less than if the book had been bound, will prove prohibitive to most private individuals, and English readers will be disappointed that, apart from the botanical sections, the text is in Swedish, though with a commendably full German summary.

But what of the human and cultural interpretation that Vrå calls out for? On what impulse, by what route, did these early Neolithic farmers penetrate and settle so far away from their nearest kinsmen? What were their relations with other groups and what their subsequent history? Does not Vrå offer a clue to the genesis of the E. Swedish pitted-ware cultures? These problems are hardly dealt with at all, giving a feeling of sterility and deadness to this materially superb contribution. Perhaps these questions will be illuminated by the other major Swedish Neolithic site whose publication we eagerly await—Alvastra. The problems of the primary and secondary cultures of Sweden may be nearing solution; Florin's work on Vrå is an important landmark on the road to this final goal and we welcome it gratefully.

STANLEY THOMAS

REVIEWS

ADVENTURES WITH THE MISSING LINK. By RAYMOND A. DART with DENNIS CRAIG. London: Hamish Hamilton, 1959, pp. 251, 20 half-tone plates, 21 line drawings. 25s.

Although this book is written in a very readable non-textbook style, it should be of as much interest to the specialist as the general reader. Professor Dart, with the assistance of Dennis Craig, sets out to describe in detail the histories and controversies associated with the Australopithecine sites and fossils in South Africa, especially those of Taungs¹ and Makapansgat with which he has been directly concerned. There are also numerous digressions which, for once, add to the readability of the book and give one more insight into the personality of the senior author.

Most readers who are interested in early man will know something of the discovery of the first Australopithecine skull by Dart in 1924, but few will remember, and fewer still know about, the considerable differences of opinion as regards the affinities of this specimen. Dart had to be patient for many years, in fact until further discoveries by Dr Robert Broom, before his ideas were more generally accepted.

In assessing Dart's contribution to the study of Man's fore-runners, it seems to me of importance to have our memories refreshed on these historical details.

The book may be divided roughly into two sections. First of all he deals with the discovery and impact of the Taungs skull. It is to Dart's everlasting credit that he quickly perceived the hominid affinities of this specimen, even though it was an immature individual, and maintained his opinion through considerable criticism from specialists, including Sir Arthur Keith and Sir Arthur Smith Woodward. This is not to say, of course, that the scientific criticism was not a healthy approach to a newly discovered fossil, especially as the overall form of the skull face is fairly similar to those of some immature great apes. Indeed, it is not until more specific features, especially of the teeth and endocranial (brain) cast are considered that the hominid affinities of the skull become more evident.

The latter part of the book is concerned with the so-called Osteodontokeratic (bone-tooth-horn) culture. The careful analysis of over 28,000 animal fragments from the Makapansgat site by Dart and his colleagues suggested that bones were used by these early hominids and that there was a selective utilization of certain bones only. Moreover, they claimed that these primates modified the bones by certain tool-making techniques. Again Dart was belaboured by critics, especially by those believing that broken bones in caves were the work of hyenas or other carnivores, and on this point there is still considerable difference of opinion. The discovery of flint tools at the Sterkfontein and Makapansgat sites in South Africa and at the East African Australopithecine site, is very strong supporting evidence that these creatures were sufficiently advanced mentally to uphold a simple tool-making culture. Yet more evidence of their use of implements (or rather weapons) was afforded by a series of baboon skulls, the majority of which had been fractured by well-aimed blows from the front.

Whether Dart is right in thinking of the Osteodontokeratic culture as preceding the Lithic phases of early man is also still debatable. In the very earliest ape-man, the utilization of whole bones and stones seems just as likely, and with the development of tool-making techniques both types of material would be of value, and it must be remembered that in both the Australopithecines and the almost contemporary Peking man the bone artefacts were associated to some extent with worked stone. Therefore, there is no evidence as yet that the very earliest hominids did purely utilize bone.

It is refreshing to find this fossil group generally discussed under the single generic

¹ Some anthropologists consider the correct name to be Taung.

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title of *Australopithecus*, for there is every reason to believe that the five *Australopithecine* genera so far described—*Australopithecus*, *Plesianthropus*, *Paranthropus*, *Telanthropus* and *Zinjanthropus*—should in fact only be recognized by specific or even sub-specific names.

There are twenty pages of half-tone illustrations (unnumbered), all of a high quality. The standard of the twenty-one line drawings varies considerably, those by Papas being in a very pleasing style, although perhaps rather imaginative. In a number of instances half-tone illustrations were found in a different part of the book from that referred to in the text.

DON R. BROTHWELL

APPROACH TO ARCHAEOLOGY. By STUART PIGGOTT. *London: A. and C. Black, 1959.*
pp. 134, 8 plates, 12 figures. 15s.

This new book by the Abercromby Professor of Prehistoric Archaeology at Edinburgh is the outcome of a course of lectures given in the winter of 1957-8 to teachers of history in Scottish schools. Its object—to make an educated lay audience appreciate the possibilities and limitations of archaeology as a discipline—is realized with grace and intelligence and the result is an attractive and thoroughly worthwhile volume. The difficulty of writing such a book, all the greater for one as closely involved in the process of recovering and interpreting the raw material of archaeology, is to steer a course between professional jargon and over-simplified explanation. Stuart Piggott has solved his problem by addressing his readers in the easy conversational style of a practised performer on television. The result is unaffected and commendably rapid communication.

The book opens with an essay in which emphasis is laid on the status of archaeology as a sober historical discipline, an emphasis in which one seems to detect some anxiety to counter the false impressions imparted by the exploitation of the subject in a variety of popular media. It might be objected that in fact Piggott over-emphasizes the uniqueness of the role of archaeology as a way of reconstructing the history of peoples lacking a written record of their own. Thus it is not quite clear to the reviewer why information about prehistoric communities obtained by means of archaeology is any more 'direct' than that gained by any of a variety of other disciplines. Indeed, the opposite is often true. What could be more direct than the insight into early husbandry given by the identification by botanists and zoologists of grain imprints or animal bones? By comparison, artifacts like sickle-flints or rubbing-stones allow no more than inferences; and these are not merely indirect, but commonly ambiguous.

Succeeding chapters deal respectively with the methods and techniques used in recovering and studying archaeological data and with the ways in which chronological scales are constructed for the long periods uncharted by historical dates. Other chapters explore more fully how the archaeological method can be applied to increase our understanding both of non-literate, prehistoric societies and of those illuminated to some degree by written documents. Professor Piggott emphasizes how limitations in the evidence restrict the information to be obtained by means of the archaeological method and, more fundamentally, why it is more successful for some levels of human experience than for others. In summarizing his conclusions he emphasizes the need to bring all possible sources, humanistic and scientific, to bear on the task of widening and deepening our knowledge of the past. His book, which many others than the fortunate schoolmasters will wish to possess, is rounded off by well-considered suggestions for further reading.

GRAHAME CLARK

REVIEWS

GREEK PAINTED POTTERY. By R. M. COOK. London: Methuen, 1960. pp. 391, 56 plates, 43 text figures. 63s.

This is the first comprehensive handbook on Greek vases to be published in any language since Pfuhl's great work of 1923 (*Malerei und Zeichnung der Griechen*). Rumpf's brilliant study, which appeared in 1953 in the *Handbuch* series, covers a broader field but in less detail and with a different purpose. Now for the first time Greek vase-painting is better supplied with an authoritative text-book than either Greek sculpture or Greek architecture. To many who do not know the author's work or whose experience of Greek vases is conventional the scope and balance of the book may seem strange. Athenian black- and red-figure occupy less than one-fifth of the pages which are devoted to the different wares, and the regional archaic schools nearly one half. The treatment of the Athenian vases is sympathetic of their high artistic merit, but it shows how much what one might call the archaeological study of these vases has lagged behind the art-historical expertise which they have long and understandably commanded.

The sources for the subject are by now legion, and yet for some neglected classes the author's comments afford the only available or up-to-date treatment. He is throughout fair in his explanation of the views of others. The occasional note of rather cynical disbelief or scepticism enlivens the reading and will offend few. He is always at pains to point out where evidence is incomplete or possibly misleading, and the dust-cover advertisement claims with some justice that 'the style is clear and uncluttered by professional or art-historical patter'.

In the main discussion the five stylistic periods—Protogeometric, Geometric, Orientalizing and Black-figure, Red-figure, Hellenistic—are sub-divided by region or group, and for each group the reader will find an account of shapes, technique, decoration and connexions with other wares; no digressions. There is a worthwhile chapter on black-painted and relief vases; another—under-illustrated—on shapes; and two good chapters on technique and inscriptions. The problems of absolute chronology are reserved for a separate chapter, and this is a reasonable arrangement for the fixed points are few and many are of debateable value. 'The Pottery Industry' introduces an account of the role and identity of potter, painter and manufacturer, and a review of what little is known about prices. The 'Uses for other Studies' briefly acknowledge the importance of the content of the vase-paintings—faint praise which might have been omitted, while the remarks on the possible historical value of properly identified and dated vases lose their effect without more examples being quoted. Practical comments tell the student how to hold a vase, look at it, take notes, draw and photograph it, clean it and mend it (the picture on the dust-cover is a good example of inadequate modern restoration of fine drawing, but it was not perhaps chosen by the author). The long last chapter on the History of the Study of Vase-painting will fascinate the expert but is ill-balanced in the whole work.

There are generous reader's aids: full bibliographies for every section with candid comments on each item, glossary, note on museums and index. The plates are carefully chosen and well reproduced; in the text figures the line is sometimes weak or coarsened. The price is fair.

It would be idle to pretend that all the opinions offered in the book are wholly acceptable, but improper to pick on details here. Some foreign scholars in particular will find favourite and even recently expressed views about some archaic wares flouted, though not ignored; but in these matters Cook is not easily gainsaid, and by his fair presentation of the facts he has obviated much criticism. Non-classical archaeologists may congratulate themselves on the comparative simplicity of most other ancient wares, but envy the intrinsic interest of the Greek vases and the degree of classification and dating possible. In all essentials this

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is a reliable historical account of Greek vase-painting which can safely be recommended to students, and with which experts will rarely find exception; and there are few handbooks on any subject of which so much can be said. Both author and publisher deserve our thanks.

JOHN BOARDMAN

THE VIKINGS. By JOHANNES BRØNDSTED. *Pelican Books* (A459), 1960. pp. 300, 24 plates, maps. 5s.

Even if this were no more than the brief popular account which at first it may seem, it would still be welcome as the first to combine a clear sketch of the confused history of the three centuries concerned, an account of the Vikings' art and culture, and an archaeology of the period with a good selection of illustrations. The author devotes about a third of the book to each of these main elements. But it is much more than a popular sketch and often gives material not previously available in English. It is inevitable that each third when compared with earlier specialized volumes should seem hurried, but it never seems superficial, and there is an excellent select bibliography (of about 200 items) which is a great advance on anything available elsewhere. In some chapters the author allows himself a little more space; that on 'Towns and Earthworks' must be of general interest for its unparalleled picture of town life in early Northern Europe, based on recent work at Hedeby, Truso, Birka and Slesvig. The chapter of similar length on 'The Viking Way of Life' successfully avoids the pitfalls of such a title to give a well-documented (and notably unenthusiastic) account.

There are indeed only two qualifications of my otherwise unreserved admiration for the book. The first concerns the use of literary sources. The excellence of those for this period may be thought their most dangerous temptation, but it seems to me a failing in Dr Brøndsted's otherwise comprehensive account that he is rather distant where they are concerned. An instance is his view that the distinction between the *hafskip* (beamy ocean trader) and *langskip* (narrow warship) dates only from the end of the Viking period. But King Alfred described his new hull shapes (of greater displacement than the *langskip*) as 'neither of the Frisian pattern nor the Danish' and it seems clear to me that he was using these to distinguish the *hafskip* and *langskip* typical of his day's merchants and pirates. Where beliefs and attitudes are involved the rather arms-length use of literature is even more dangerous; the *Volga-thattr* described as an Eddic poem (and thus of the Viking period) is in fact a medieval peasant tale in prose.

The other regret concerns the translation, which is occasionally merely irritating to someone who already knows the material, but may be puzzling to those who don't. A reference to 'dragon's cave' should be 'snake-pit', the 'playing-bricks' are in fact chessmen, and so on. This is, I suspect, the sort of mistake which a translator can often only avoid by knowing the subject as well as his author; the terms used are often ones that would have been reasonable alternatives had English not already decided on its equivalent form. The point is trivial for those who recognize Roar as the Hrothgar of *Beowulf*, or Henry Bird-catcher as Henry the Fowler, or lovsigemand as logmann, but what about those who don't?

These two points apart, the book commands admiration as well as respect, and those statements which may seem hard to swallow (that Anglo-Saxon art is a mere copy 'with little originality' of Irish!, or that Ringerike style is only 11th century) are at any rate very interesting to ruminate upon.

A. L. BINNS

REVIEWS

RUNES. An Introduction. By R. W. V. ELLIOTT, M.A. *Manchester University Press*, 1959. pp. 124, 24 plates, indices. 30s.

The reader interested in runes, but restricted to English, has hitherto been confined to *Scandinavian Archaeology*, *Shetelig*, *Falk and Gordon*, *O.U.P.* which gives what is still for its length the best survey of these fascinating symbols. His only further step was into excavation in Stephen's magnificent but undisciplined and unreliable folios. The introduction under review will enable such a reader to follow up more fully the matters compressed in *Scandinavian Archaeology*, and gives a neatly ordered survey of the varying theories of runic origins and of their names and use. The distribution maps of those English and Continental inscriptions mentioned in the text, and the five comparative tables of forms and names are clear and of great practical use.

The book deals with selected English runic inscriptions in detail, and refers to the more important Continental ones. In view of the large proportion of the runic inscriptions of the British Isles which is of Scandinavian origin it is a pity that none of these is included in the runic text, and only one (the Lincoln comb) illustrated. The absence of runic text and translation for this and some of the other illustrations must reduce their utility to students. This is important, as the quality of the illustrations is extremely uneven, and about a third (which must have added appreciably to the cost of the book) are technically unforgivable. It is a difficult and time-consuming business to produce photographs which show clearly such slight markings on stones, but the best examples in the book (from ATA Stockholm and the British Museum) show what can be done. Some of the unfocused, under-exposed work with barely contrast to hint at an inscription is inferior to the honest 19th-century drawings, and must make one regret that the author did not apply to his illustrative material the rigorous standards of selection apparent in his own text.

It is natural that in an introduction as comprehensive in scope as this the author should not seem equally at home everywhere. He puts a stress which seems excessive on connection between early runic inscriptions and 'pre-runic' (surely only in a temporal sense?) hällristningar; the sound value [A] for the *a*-rune is puzzling, and at variance with common views on Germanic *a*; the view that the Old English poems *Wanderer* and *Seafarer* 'pay only as yet lip-service to Christianity' seems to me gravely misleading. But these are all arguable matters, and with few exceptions the *Introduction's* blemishes are all of this kind, and need not detract from its welcome as a most useful and, on its own field of runology, reliable book.

A. L. BINNS

HISTORICA

Welcome to *Historica*, a new yearbook published by the Historical Institute of the Czechoslovak Academy of Sciences. It is intended that this yearbook should contain the most important results of research work done in Czechoslovakia in all fields of historical research investigation. All contributions are published in either English, French or German. The chief Editor is Dr Josef Macek, and the address: *Publishing House of the Czechoslovak Academy of Sciences, Vodičkova 40, Praha 2*. The first number, published in 1959, includes in English a long article by Josef Poulik on 'The Latest Archaeological Discoveries from the Great Moravian Empire', and a short account by Bohuslav Novotny on the '*Limus Romanus* in Slovakia and the Future Tasks of its Investigation.'

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